PERMANENCE AND EVOLUTION

BOUVERIE-PUSEY

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AN INQUIRY INTO THE SUPPOSED MUTABILITY OF ANIMAL TYPES

BY

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PREFACE.

It may seem almost presumptuous on the author's part to attempt to reopen once more the whole question of evolution, especially as in doing so it is necessary to call in question the views of so many very eminent men of science.

At the same time, any one who calls attention to any neglected facts, or who questions assumptions too carelessly allowed to pass muster, helps to elucidate the subject of which he treats, and so aids the cause of scientific knowledge, whether the particular views he propounds are right or wrong. Bearing this in mind, and observing, or thinking I observe, that while the hypothesis of evolution is almost universally taken as proved, and while fierce war is waged over its details, the primary grounds adduced for its support have been accepted with far too much facility and too little investigation. I have, therefore, published some reflections upon facts within my knowledge, which may tend to exhibit this important subject in a new light.

I have endeavoured to keep a broad distinction between evolutionism in general and Darwinism in particular. An evolutionist is one who believes it to be scientifically demonstrable, or at any rate to be much the most probable hypothesis, that living types originate by development the one out of the other, under the influence of agencies at work now, and which we are capable of knowing now, or may expect to know in a reasonable time. A Darwinian, in addition to this, believes that the sole or principal agency by which living types are evolved, is natural selection, or the survival of the fittest (that is, the presence of a general tendency to vary indefinitely, coupled with the gradual accumulation and intensification of such variations as happened to be favourable to the organism), either coupled or not with sexual selection.

The hypothesis of evolution in general, and the special Darwinian form of it, stand on very different bases, and require very different modes of treatment. It is the object of this work to show, that while Darwinism proper is improbable, evolutionism in any form is as yet unproved; while, on the other hand, the more we investigate the facts of inheritance, the more we are compelled to regard differences so slight, that they would usually be considered casual variations, as within the limits of our existing knowledge strictly permanent.

The animal kingdom only is spoken of in this treatise, as I have no knowledge of plants. The most superficial acquaintance with the frequent sports and new varieties of cultivated flowers, etc., is enough to show that vegetables exhibit an amount of variability far greater than anything we have any reason to believe existing among animals; yet I cannot but think that, *mutatis mutandis*, the general principles which in the ensuing pages I attempt to trace out, will be found applicable to the whole of organised nature.

What I am endeavouring to establish is not the assertion or negation of any particular views as to the origin of organic races, but on the contrary the conviction that the whole matter is beyond our present ken. But if I were asked in what way the subject could be brought within the area of possible science, I should say that two things must be done.

The first, and by far the easiest, of these tasks is to find out by careful experiment what changes really can be produced in a pure strain, and how, in the same way as we have ascertained the behaviour of chemical elements and compounds. This requires trouble and patience—little more.

The next requisite, of even greater importance, is vastly more difficult to obtain, for it involves some knowledge on our part of the mode of action, if not of the nature, of the forces by which the formation and precise arrangement of the several parts of the mechanism of an organism are determined, as well as of those laws under the influence of which one creature, say, is caused to be of one colour, while another, in other respects very closely allied, has a different colour imposed upon its corresponding textures, and so with regard to other characters.

Until such knowledge is obtained, it is difficult to see how any intelligent conception of the relative importance of the various zoological characters of living forms can be reached.

We are scarcely on the threshold of such knowledge at present, and as long as this is so, I confess it seems to me little more than beating the air to throw out conjectures as to the origin of organic races.

PERMANENCE AND EVOLUTION.

INTRODUCTION.

THE notion of genus and species in the animal kingdom originates with Linnæus, as also does that of unity of descent as connected with the latter. I think he was brought to this doctrine by considerations of a dogmatic kind. Linnæus, who was an orthodox Lutheran, finding in Genesis that all living beings were created after their kind, and finding elsewhere in Scripture that all races of men sprang from one pair, presumed that all animal types logically equivalent to the types of man, also sprang from one stock. But gradually, where Linnæus had seen only identity, subsequent investigators saw unity in diversity. It was soon found out that where Linnæus only saw the pheasant there were many permanent types of pheasant, and gradually it was doubted if all domestic animals bearing the same name originated from one stock, and the same doubt was extended to the races of man.

At first these doubts only attached themselves to the better marked breeds of domestic animals and the principal races of men, but the authors of "Types of Mankind" saw that, *e.g.*, the fair and dark races of Europe were equally permanent with the Negro or the Hottentot, and Agassiz maintained that men were created in nations.

Meanwhile these views were partially applied by Broca (articles in "Journal de la Physiologie de l'Homme et des Animaux"), Brent ("Pigeon Book," &c.), from the point of view of a practical breeder, and the Rev. T. Dixon ("Ornamental and Domestic Poultry and Dovecot and Aviary"). This gradual development of opinion in the direction of a more and more thorough polygenism seems to me to be right, at any rate provisionally, during our present state of knowledge, or rather ignorance. Further investigation would no doubt have led to an increasing and still increasing recognition of distinct permanent forms, if it had not been that none of these investigators had a clear idea of what is really meant when breeders are said to improve or alter a breed.

While public opinion was in this state, Darwin's "Origin of Species," and the simultaneous labours of Wallace, took the scientific world practically by storm. Since then (with the exception of the remarkable protests of Sanson, "Zootechnie Econome du Bétail," as a polygenist, and of Wigand of Marburg, "Darwinismus," virtually as an adherent of the old notion of limited variability), though not all naturalists have been strict Darwinians, yet nearly all have been evolutionists of some kind or other; they have all believed that species, genera, and larger divisions originated the one from the other, by the agency of known causes acting now, which is the main point. A man who fancies that they were transmuted through causes not now in operation, is not a real evolutionist, but an advocate of permanence, who, to his scientific belief in permanence, chooses to join an odd cosmogonical guess, avowedly outside the limits of induction. While these new evolutionists are really the extreme opposite of the American polygenist school which immediately preceded them, in some important respects they have an odd agreement, so that many (Broca, Vogt "Lectures on man," Ed. Hunt, Blyth, "Annals and Magazine of Natural History"[*]—"Land and Water,"[†] early volumes) passed from one to the other. They both agree that what are called varieties and species differ in degree only; but one school made varieties lesser species, the other made species (not to say higher divisions) only better marked varieties. They both had the tendency in common to oppose the confused and conventional notion of species (though Darwin is reduced by the necessities of his argument, while in general making the distinction appear small, when he wishes to prove that pigeons or fowls are not derived from several stocks, with great inconsistency to build up the very distinction he has before been pulling down). Still, in general, the two agree in that, and whatever else becomes of Darwin's labours, the conventional notion of species, which includes together all domestic cattle and all human beings, and at the same time separates the wolf from the jackal, can never be built up again.

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[*] First Series, vol. xix. p. 102.

[†] Vol. ii. p. 110.

In fact, the conventional idea of species is the antipodes of a clear idea, combining, as it does, the notions of origin from one stock, real or possible; existence as distinct races in a wild state, even in separate areas; fertile reproduction *inter se*, breeding together naturally, without human interference, and a certain undetermined degree of likeness. That these things go together is taken for granted, hence interminable debates as to whether any given races of animals are distinct species, or only varieties of one species. It is often brought forward as proof of the specific unity of two animal forms, that when crossed they produce perfectly fertile offspring. Now if we adopt the definition of species founded on fertile interbreeding, this is tautology; if any other, it is an unproved assumption on a subject of which we can know nothing except from experience. There is no reason why there should not exist in nature any number of original races differing in all manner of degrees, and capable of producing crosses of all grades of fertility, from zero to perfect productiveness. It is owing in great measure to the element of descent in the conception of species that an importance is attached to it different in kind from that belonging to the conceptions of family, order, etc.; species represents, it is said, a matter of fact, the others are only arranged as a question of convenience. Going on this principle, and no rule being known for determining what differences are permanent and what not, when dealing with wild animals, every, or almost every, distinguishable race has been put down as specifically distinct (*Asinus onager* and *Hemippus* for example), and when dealing with domestic animals, races are placed as of one species, which, if wild, would have had no chance of being so placed (the European and Indian cow for example).

It is to the credit of the evolutionists to have done much to end this state of things;

to have pointed out, for instance, that the existence on the two sides of Behring's Straits of the American and Old World forms of fox, and the American and Old World forms of wolf, are to all appearance facts of the same kind. But a clear and firm conception of this is just as consistent with a tendency to make stricter as to make looser our notions of permanence of type. There is no difficulty in believing that at the first formation of animals there existed independently, not only what we happen to call distinct species, but also subbreeds, and strains still more faintly characterised.

It may be taken as generally known and admitted, that the power to produce fertile hybrids often does not run in close conformity with morphological affinity. The causes of sterility are in the present state of our knowledge very obscure. We know that some fertile individuals of the same species are incapable of breeding with one another, as Darwin points out ("Origin of Species," Edition IV., p. 302, *et seq.*). It is, then, rash to feel sure that the difference between races which will not cross, not only so as to produce sterile offspring, is deeper or more fundamental than that between some which produce fertile crosses, for it may be that it only happens to affect the reproductive system and not any other part of the organism. Even granting it is more fundamental, it does not follow that permanence of type is less absolute in the one case than in the other. Most of the disputes there have been as to the specific unity of different animals arise out of the appropriation of parts of the definition of species by naturalists, who have ignored the remaining parts.

For instance, when De Blainville ("Osteographie") found that the differences between the several Eastern forms of wild hog were small compared with what he was accustomed to find between forms classed as species, he forthwith made them one species, taking the purely morphological ground of determination founded on a certain undetermined degree of likeness. Thereupon he supposed those Eastern races to be of one origin, and the descendants of escaped domestic pigs. So the eminent anatomist John Hunter supposed the dog, the wolf, and the jackal to be necessarily descended from one stock, because the dog would breed with both the other two, and the offspring would breed again with the dog; whilst, on the other hand, the so-called splitters start from some other point in the definition of species, as distinct existence in a wild state, for example, and sink the morphological side of the notion altogether, and with it the æsthetic symmetry of a natural system. In disquisitions as to the specific unity of domestic breeds, species means little more than wild stock.

I think it would be best, in the present imperfect state of knowledge, to drop altogether the use of a word which has become so pretentiously indefinite, and which savours so much of the empty and misleading abstractions of the schoolmen, and to limit ourselves to facts known or ascertainable—to speak of races, breeds, or forms, and to ascertain directly or by analogy the permanence or impermanence of each separate breed or of each particular character, with as little importation of doubtful and extraneous consideration as possible.*

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* So Huxley, Proceedings of Zoological Society, 1880, p. 286. "As for species no one zoologist has ever yet agreed with the estimate of another as to what should be considered species, and what local varieties among Wolves and Foxes ; and as there is no criterion by which the question can be decided, it is probable that such agreement can never be attained. The suggestion that it may be as well to give up the attempt to define species, and to content oneself with recording the varieties of pelage and stature which accompany a definable type of skeletal and dental structure in the geographical district in which the latter is indigenous, may be regarded as revolutionary; but I am inclined to think that sooner or later we shall have to adopt it."

DARWIN AND LYELL: COMPARISON AND CONTRAST.

THERE is a great difference in kind between Lyell's hypothesis (if hypothesis it can be called) of the unity of past and existing geological causes and the hypothesis of evolution. The former has to do almost entirely with mechanical motion, and the causes producing change are, in the case of aqueous agents, exactly—in the case of igneous, approximately known. In the other case, both effects and causes are infinitely complicated and ill-understood. There is a great fallacy in putting things quantitatively, which are in truth matters of kind. It is said that species differ far more than varieties and varieties than family strains, and that this greater difference is the result of lapse of time ;

but genera, if properly established, will be found to differ not only more than species, but in different respects (*see* Wigand, Vol. I.), and so species as compared with varieties, if the distinction can be maintained. Each case must stand by itself. The burden of proof must always lie on the asserter of mutability, for the permanence of the forms of organised nature that which is first impressed upon the casual observer, and that which additional inspection only serves more and more thoroughly to demonstrate. Granting that it is proved that colour can vary independently of reversion, this proves nothing as to the shape of the limbs nor that last as to the dental formula.

Darwin says that some authors have asserted that an important part never varies, and that the same authors rank only such parts as important as never vary; but even so there would be no arguing in a circle, for at any rate we should be told that there were some parts which never vary, that variation has its limits. But this is not at all the whole of what is probably meant. In the first place, we must not consider only or mainly the points of the topography of the organic system in which differences may happen to be located, but also much more the nature of the differences themselves, just as in some diseases it is only the unscientific who classify them according to the parts of the organism which they may attack, *e.g.*, all eye diseases, all affections of the chest, etc. Secondly, such distinctions are important as go along with and serve as an index to pervading differences running through the whole system, *e.g.*, differences in the dental formula; those on the other hand are unimportant which exist between forms in all other respects identical or nearly so, *e.g.*, differences in colour. Granting, then, that natural selection with spontaneous variation could within the period of history develop out of the rock dove a fantail, I do not see how we are any nearer the conclusion that in ten times or a hundred times that period, these causes would develop the Goura pigeon; granting that, that a millionfold as much time would evolve any of the true Gallinaceæ. Lyell only pointed out that if a given cause elevated the bed of the sea a foot in a century, the same cause continuing a million years would elevate it 10,000 feet. If we suppose the case to be strictly parallel, we must suppose that as selection, natural or methodical, has in 200 years lengthened the carrier pigeon's bill half an inch, so in 800 it might be lengthened two inches and so on. Whatever we may think of this argument, at any rate the thing known and the thing supposed are analogous in kind; but if we, knowing only that the bill could be lengthened, presumed with no other evidence that its colour could be altered, we should be really cutting loose from experience altogether.

To give an instance. The European cow and the humped cow differ according to Blyth (cited Darwin, "Animals and Plants," vol. i. p. 79) in the shape of their ears, in the point where the dewlap commences, in the typical curvature of their horns, etc., and in the one being born with teeth protruding through the jaws, the other not so; not to mention the presence or absence of a hump and important osteological differences mentioned by Rütimeyer (genus Bos). From these reasons, Darwin conceives that they must have originated from separate wild stocks; but yet of course, in accordance with his fundamental hypothesis, supposes that at some period they diverged from one original form. But I do not see how in this case the supposition of more time is of any use. If we knew that these characters were producible in a small degree, there would be some sense in the supposition that in a sufficient length of time they might be increased to a vast extent. But as we have no evidence that they vary in any degree, either in cattle or in any other animals, I do not see of what use immense periods of time would be to the theorist. Darwin in one place ("Animals and Plants," p. 233) seems conscious of this, and says that it has been objected that the differences between domestic pigeons can throw no light upon those between wild Columbæ, because they do not differ in the same respects; to which he replies that man would only select such differences as were externally conspicuous. But, if so, where is the positive evidence of the other modifications being producible? I do not well see how we can have any, and therefore in those cases the general presumption of permanence must prevail. There is also perhaps no reason why granting natural races to be modifiable at all, they should all be modifiable in exactly the same degree.

Another question here suggests itself. When we are dealing with the gradual upheaval of land from sea, we do not scruple to suppose slow processes of rise or fall, which we know to have gone on for hundreds of years, to have gone on for thousands or millions; but it is doubtful whether we may make an analogous supposition as to living organisms. We are only justified in going beyond the naked results of experience when we understand the laws by which the part of nature we are dealing with is regulated. Now this we know, speaking generally in proportion to the degree to which these laws are simple. But nothing in nature is regulated by such a complex of little known laws as a living organism; therefore, it is much less safe to travel beyond the mere data of experience here than in any other departments of nature, which are simple and better known. No one who was employed in sawing through a granite column, and who found that he got through an inch in a month, would doubt the possibility of his getting through a foot in a year; but we should think rather ill of a physician who, finding that a patient who when he first came to him could only walk two miles an hour, at the end of six months could walk four miles, should presume that at the end of a year his patient would be equal to eight miles per hour, and at the end of two years to sixteen miles.

A living organism is more like a machine than like anything else we know of, and we know that machines can be played tricks with in certain respects up to a certain degree, and yet continue to go on working; while in other respects, and to more than a certain point in any respect, they totally refuse to be interfered with. It seems then that the principles we ought to go upon in discussing evolution are these:—

First, to find out by carefully recorded and sifted experience what characters, if any, are liable to variation, and in what respects, care being taken to eliminate the effects of reversion.

Secondly, when these are found, we may to a certain extent generalise so far as to suppose in the same or closely allied natural groups the possibility of a moderately greater amount of change through the same causes acting through longer periods. As to all beyond, we must suppose permanence to prevail, having no reason to think the contrary. Our instances must naturally be sought almost entirely among domestic animals.

In one chapter of Darwin's "Domestic Animals and Cultivated Plants," he enunciates truths which tend to the overthrow of his whole hypothesis. I mean those in which he speaks of the power of inheritance, and especially calls attention to the fact that characters can be "fixed," as for instance when (Vol. II. p. 24, Edition II.) he says that no character is more fleeting than colour, especially in the horse, but that it appears that with careful selection fixed races of uniform colour may be formed. What is this but to say that colour in the horse is not fleeting but permanent, and that if it appears otherwise, the cause lies in the absence of selection, *i.e.*, in intermixture. We must take care not to be deceived by words; there are in nature permanent races so much alike that they intermix naturally when found on the same area; for the breeding together of these we have no name, breeding them apart we call applying selection. There are other races somewhat more distinct which only breed together when secluded from their own immediate kind; for the separate breeding of these we have no name, but their commixed breeding we call crossing. Another fallacy of language consists in giving an independent existence to abstractions, as in saying, "the horse is very variable, the ass less so," which means the type we call horse exists conjoined with much more diversity than the type ass. The facts of inheritance are so well known, and those of reversion so well established, that we are entitled, or rather compelled, to treat any case of apparent non-inheritance as due to reversion till the contrary be clearly shown.

To obtain absolute demonstration on this point is probably impossible. It is only of a few privileged animals that pedigrees are kept; and there is no certainty exactly how far reversion may operate, but if we find that most characters of animals are accounted for by direct inheritance, that of the remainder, a large proportion, can be immediately shown to be the result of reversion, and that in proportion as the circumstances are more distinctly known, this more and more appears to be the case, we are justified in presuming that inheritance is at all events at present within the sphere of our knowledge absolute, and that the small residuum of instances which we cannot resolve is due to our ignorance or mistake. For it can never be enough repeated, the real question between the adherent of evolution and the opponent of such views, is not properly whether organic races were evolved out of other organic races, or out of some other form of matter, or produced in some other way, but whether or no organic races were produced by means we can understand or make familiar to us with our present knowledge.

I do not say that existing races were not evolved out of a smaller number of forms, but feel sure that (if they were) some agency was at work which is either not at work now, or else carefully conceals itself from view; and I feel also individually convinced that nobody knows anything, or is likely soon to know anything, as to the mode of their production. If I am asked whether this implies that the production of living races is for ever beyond the ken of science, I answer by no means, for the motions of the heavenly bodies were equally inexplicable before Newton and his immediate forerunners. But as it was the duty of a scientific man to maintain the unfoundedness of the Cartesian vortices and similar pseudo-explanations, based on loose analogies and on no real evidence, though himself unable to explain the facts, or even in any way to contribute to or forsee their explanation, so in my opinion is it with Darwinism, etc., now.

Of other forms of evolutionism I can hardly speak so much, as they scarcely even profess to be scientific hypotheses or more than loose conjectures. Lyell's resolution of past geological phenomena into the effects of existing causes, has often been supposed to afford a parallel to the doctrines of modern evolutionists, but the things are different, not to say extremely opposite. Lyell collected with great care a vast number of instances of the actual operations of aqueous and igneous causes, all carefully and accurately recorded and investigated, and depending for the most part on perfectly well known mechanical laws, entirely expressible in terms of more or less; and he proved, or rather he pointed out, that only suppose these operations to have gone on long enough, they must have produced appearances similar to those we see in the earth's crust. Here, then, was nothing which was not clear, closely reasoned, and scientific. How different when the matters spoken of are, as I showed before, not properly quantitative, and still more when the modern changes adduced are entirely conjectural. In science we have nothing to do with what is probable as a basis; we may form hypotheses as guides for investigation, or to group together instances, but our bases must be proved facts. One man's probable is another man's improbable.

Lyell no doubt at times used incautious language, as if the supposition of causes having acted at one time of the world's life, and not at another, was in itself anti-scientific, a view hardly reconcilable with present astronomy; for the doctrine of the secular cooling of the solar system seems to imply an absolute beginning of force, in kind differing from anything we know to exist now. But there is nothing unscientific in saying, "Here is evidence of a cause not now in operation as far as we know," provided we do not attempt to define this cause except negatively. It is merely a question of evidence. The reason why the causes assigned by sixteenth century cosmogonists for the origin, *e.g.*, of fossils, "stellar influence," "a fatty matter," "a lapidifying juice," were absurd, was not only or mainly because they were supposed to operate formerly and not now, but because they were themselves vague and assumed without evidence. The cosmogonists would not have made their case much better by maintaining that the lapidifying juice or the like, were making shells at the present day also.

The agency, whatever it is, by which species, varieties, etc., were formed, seems to have been at work on this planet up to and during the immediately post-tertiary period, and not later, as far as we have grounds to believe. Of what nature it was, what laws it followed, why its manifestations have not been apparent of late, whether it will ever wake up again, are matters about which we know as little as about whether its operations are limited to this planet, or extend through any or all of the other globes in the universe. The whole matter lies far beyond our present ken.

Now, speaking with only slight exaggeration, we may say that no facts have been alleged in favour of the origination of new specific or race characters at the present day. I do not mean only that no such instance has been proved. I say almost none has been alleged. It has been said that there are these and the other domestic breeds, and that it is unlikely that if they existed in a wild state they should have been exterminated; that such and such animals have been in such and such ways modified by transportation to America—that is, that some observer has seen races in America which he does not know how to parallel; that "the climate of the Moluccas appears to cause the duck to vary to a remarkable extent," which means that in the Moluccas are found a remarkable number of races of duck; that such and such characters are variable in such and such breeds; that is, that strains otherwise alike exist differing in these respects. What is wanted is evidence that at such and such a date of such and such a strain kept so as to eliminate reversion, were born offspring exhibiting such and such new characters. Bearing in mind these principles, I will attempt, at the risk of some repetition, to go through the portions of Darwin's work referring to each of our principal domestic animals.

THE DOG.

It is admitted by Darwin that the dog has in all probability descended from more than one wild stock. In many countries the dogs are indistinguishable from the wolves or other wild canines—dogs are often kept by savages who have no other domestic animals; many wild canines are very tameable, and no argument can be drawn from the alleged sterility of hybrid wolf-dogs kept in close confinement. But besides these there are other breeds, the most common in Europe, very distinct from any known wild animal. Darwin says that they are "unnatural forms," and that "no one has been bold enough to maintain that they existed in a state of nature;" but seeing how true they breed, I do not see why the principal and best marked (the greyhound, the mastiff, the terrier, the spaniel, etc.) should not have so existed, and the others have been formed by crossing between them, and very likely the principal races may have existed *ab initio* in various sub-races.

It is said that in the last century "an entirely new foxhound was raised," but it is added "it is believed that this was done by a cross with the greyhound." An able writer believes that English greyhounds are the improved descendants of Scotch; 'the pointer also is supposed to have come from Spain, and is not like any dog found there now. Not much can be inferred from these facts and suppositions. Some breeds are said to have been improved by simple selection; later on we shall have a better opportunity of inquiring the meaning of this. It is said that European dogs in India uncrossed grow like the native cur, chiefly it appears in being lanky, but as it is also said that they will not continue to live and breed there, it appears that the phenomena are really those of unhealthiness.

THE CAT.

About the cat also there is little to be said. There are a multitude of small wild felines with which the tame cat will breed, and there are tame cats sometimes said to be very closely similar to them (in India it is said there are some indistinguishable from *F. torquata*). In different countries there are markedly different breeds of cats, but in the same country we find no well marked breeds, only "a considerable amount of fluctuating variability;" the explanation is that indiscriminate crossing cannot easily be prevented. Very true, if selection could have been applied we should have had many distinct breeds, *i.e.*, if cats did not habitually cross, their offspring would resemble the parents. In Paraguay, according to Rengger (Säugethiere von Paraguay, p. 42, *et seq.*), the cat, which has been bred there for 300 years, presents a striking difference from the European cat, which must mean from any Rengger happened to know. In New Zealand the feral cats assume a streaky grey colour like that of wild cats. This is one of the commonest colours in the tame cat. Where is the evidence that their colour was ever anything else?

THE HORSE.

Very little need be said about this animal, of whose wild origin little is known. There are a great number of breeds and sub-breeds, which reproduce their like with remarkable persistency, even colour, as Darwin remarks, being transmitted with great accuracy; and we see that several races of wild horses exist in various parts of the world, which have been supposed to be feral, but may just as easily be truly wild. I see therefore no ground for saying with Darwin, that few will agree with Colonel Hamilton Smith ("Naturalist's Library") in maintaining that the horse has proceeded from five different stocks. I do not see why there may not have been a great many more. I will only say a few words on the way in which Darwin tries to prove that all horses have descended from one dun-striped stock.

It appears that the Norwegian pony, a Burmese breed of pony called the Shan, and the lanky Kattywar race in India, are all often or predominantly dun-striped; besides, this dun-striped colour has been noticed in individuals of various breeds such as heavy cart horses, light South American horses, Welsh and Devon ponies, etc. Darwin asks whether it is likely that such different breeds living in different quarters of the globe should all have been crossed by any one aboriginal dun stock, and says that it is unlikely that the effects of a cross at a remote period could be transmitted through so many generations. With regard to the breeds which are predominantly dun-striped, these no doubt, however different in other respects, were dun-striped from the beginning; as to the other cases, nothing is known of the individuals in question, so there is no reason to suppose the cross by which their colour was acquired to have been remote; this is not the only instance in which Darwin speaks of breeds when he ought to have spoken of individuals. Against this we have to place the known permanence of colour in the horse; if out of a hundred horses born of parents of the same colour, only ten come wrong, and when the grandparents are of the same, no appreciable number, how then from a uniform dun-striped stock, with no tendency to revert to anything, can the birth of the first black horse, bay or other, be made conceivable? Darwin says the reason why inheritance appears more certain with the horse than with other animals, is probably only because its breeding has been more attended to. With this I heartily concur.

THE ASS.

There is less diversity of appearance in the ass than in the horse. In some countries there are well-marked breeds; of the origin of these breeds nothing is known. Of the existing known wild asses only one (*A tæniopus* of Abyssinia) bears any close resemblance to any domestic ass, but it does not follow that it is the sole origin of all breeds; in fact it seems (*see* "Blyth Field," vol. xxxviii. p. 51) that there are now in existence undescribed wild forms. The asses with extra markings are probably derived from, or crossed with, some form resembling in colour the zebras.

THE HOG.

As in so many similar cases, it used to be supposed that all domestic pigs had one origin, the wild boar of Europe. Pallas first insisted that the East Asiatic hog (called by him *Sus* *indica*) was from a separate wild stock, a proposition hardly disputed by any one who has attended to the subject, specially now we know many wild pigs nearer to it, than it is to the old European pig or the wild boar of Europe. Rütimeyer ("Fauna der Pfahlbauten," p. 3, *et seq.*, also *see* p. 179) has discovered the bones of a wild or half-wild race of pig in the lake-habitations, called by him the "marsh hog," from which he supposes the breeds of South Europe to descend. We thus have three origins instead of one, leaving the greyhound pig as a descendant of the European wild boar. But it seems very doubtful if this last can be assumed. Many of the more conspicuous differences between the wild and the tame pig of Europe are the result of abortion of organs, arising from disuse (Nathusius, "Schweineschädel," p. 99, *et seq.*), but there remain many difficulties. The characteristic colour of the wild boar seems to be seldom or never exhibited among tame pigs, nor the stripes on the young pigs in any ordinary breed, though in the Turkish and Westphalian breeds these last appear with great regularity; showing that domestication has no tendency of itself to obliterate them, and it does not seem likely that they should have been eliminated by selection for no easily divinable motive; not to speak of the much larger relative size of the wild boar's head, not only in length, but in all proportions.

About the origin then of the North European tame pig nothing is known. This race exists, or perhaps I ought to say existed, in various sub-breeds more or less distinguishable. These sub-breeds differ conspicuously in such points as length of the ears and legs, curvature of ribs, colour, etc. They bred very true; for each limited area had its own characteristic sub-breed; their characters, as Nathusius ("Racen des Schweines," p. 45) remarks, might be made more or less by selection, which does not prove weakness but strength of inheritance. In the *Sus indica* or Chinese pig, we see also many local breeds differing in the above-mentioned respects, of whose origin nothing is known. Also of solid-hoofed swine; pigs with jaw-appendages, etc., which probably proceed from reversion to some unknown form.

In South America and the West Indies there are many feral pigs descended from Old World stock. These reassume the characters which the domestic has lost from abortion and disease, and which are characteristic not more of *Sus aper* than of any other of the wild races of true Sus. Their tusks become large, their muzzles long, and their bodies bristly. Their young are longitudinally striped, as are those of Turkish, Westphalian, and some African pigs. In colour they do not resemble the wild boar, some being black, others reddish-brown, and some "black, with a white band across the stomach, often extending over the back;" this last in New Granada.

It is not known whence these pigs are derived; those of Jamaica are said traditionally to come from Africa, which derives confirmation from their resemblance (*see* above) to African pigs, and in general the slave trade has made the relations between Africa and America, specially tropical America, close. Many of the Jamaica feral pigs have a curiously plumed tail, which certainly it would have been thought improbable to derive from a separate wild stock if it had not been known that this characteristic belongs to the Indian wild boar, and to many domestic pigs in India. The Portuguese connection, both with India and Brazil, was no doubt the origin of their importation into Jamaica. Here again we find that an apparently trifling variation is really the mark of aboriginal diversity, and generally in the history of the pig we find that in proportion as we examine closely what before appeared one, reveals itself as multiple, and that apparently trifling characteristics are permanent when the strain is kept pure.

CATTLE.

Here, as usual, earlier naturalists supposed only one domestic ox—*Bos Taurus*—of which the Lithuanian bison, or so-called aurochs, was generally supposed to be the original stock; the American bison to be a climatal variety of the same. Cuvier pointed out the fundamental differences between the domestic ox and both European and American bisons; but all kinds of domestic ox, humped and non-humped, were still generally considered variations of one stock. Lately, Blyth (*apud* Darwin, "Animals and Plants," 2nd Edition, vol. i. p. 83, also "Land and Water," vol. iv. p. 10) from the point of view of a descriptive naturalist, and Rütimeyer ("Der Linnësche Genus Bos") as an anatomist, have pointed out such essential distinctions between the humped and non-humped cow, that now, probably, they are not more likely to be confounded than any of them is with the bison. Each of these is divisible into a number of races: about those of the Eastern form nothing can be said as to their origin, but as to European cattle rather more can.

There have been found in the Swiss Lake-dwellings the bones of three forms of ox, called respectively *Bos primigenius, trochoceros,* and *longifrons.* *Trochoceros* is supposed by Rütimeyer, but without positive evidence, to be a domestic variation of *primigenius,* while *longifrons* is completely distinct, thus making two original stocks; whereas Nilson considers all three distinct. *Primigenius* is supposed to be the prototype of the large Dutch or Flemish cow, closely allied to our short-horn; while *longifrons* appears to resemble the little Alpine cow—a very characteristic race.

But these identifications would seem to be rather vague, for Rütimeyer, on being shown the skull of a Chillingham Park cow, pronounced it an almost unaltered representative of *Bos primigenius, i.e.,* of the same race to which the Dutch and short-horn cow belongs. Now the Chillingham cow is a kyloe, differing from other kyloes in hardly anything but colour, therefore of a breed as different from a short-horn as from any cow in Europe. Anyhow, each of these forms may have existed in primitive times in any number of local races whose bones would not be distinguishable. Under these circumstances it is of very little use to pile up accounts of various breeds of cattle as proofs of evolution.

In many parts of the world feral or half-wild cattle are white, with red or black ears. Of the origin of these races nothing is known, but they appear occasionally to produce black calves, or calves with black or brown spots, from which it seems likely that the coloured ears are the result of a cross. All of our domestic breeds have been much improved by selection, that is, the better strains have been preserved and the worse exterminated; and here I may allude again to the nature of the so-called changes effected by modern breeders.

It is often said that breeders have changed the whole character of a particular race, and the success of their endeavours is often considered a proof of the mutability of living forms, whereas it is a proof of the contrary. First. We must part off all that comes under the head of functional gymnastics, so called by Sanson, namely, all that refers to organs which increase in power or virtue by activity, specially the muscular and nutritive system; all, in fact, that is alterable in the same adult, of the same sex, at the same season of year. This corresponds roughly with Darwin's direct effects of conditions of life and with his use and disuse. These effects, though often inherited, are not strictly race or family characters. Thus strains of various races of any animal type may be made within certain limits fatter or leaner, more precocious, agile, etc. Not that race characters go for nothing in these points, certain individuals and certain families have aboriginally more tendency to fat, etc., than others, previous to all measures of functional gymnastics; for this reason certain strains became more famous than others. And there is no reason to suppose that this quality, namely, the possession with little cultivation of excellencies, which in other strains of the same breed can only be obtained by much care, is less strictly inherited that any other quality.

The task of the breeder is thus twofold—to select such strains as present naturally the required characters; and, secondly, to assist them by judicious hygiene, if the characters are such as to admit of it, which is only the case when the improvement required is in a large sense of a hygienic nature. There is, therefore, nothing more true than the remark of Youatt, that the breeder's art is "founded on the axiom that like produces like," not on the mutability of race characters; it is in fact at bottom the art of distinguishing slight sub-divisions where previously nothing has been seen but that which is in common. So that when Youatt says that the power of selection enables the agriculturist not only to modify the character of his flock, but to change it altogether, what is meant is, the destruction of inferior members, and the multiplication of favourite strains. No doubt the first individuals selected do not exhibit the desired characters as much as their descendants will, but then it is only after selection that the desired types are freed from admixture with inferior forms. We know that in highly bred animals, of which the pedigrees are preserved, every strain has its marked characters, is in fact a little race to itself.

It is said that while the great effect produced on a breed by one superior stallion or bull, shows the force of inheritance; the fact of his superiority shows that inheritance is not absolute. It shows this only if it is assumed that all strains of one breed spring from one pair contrary to all we know in modern times, if we suppose all animals to be "created in nations," as Agassiz supposed of man, then it shows nothing of the kind. But to return to the ox. There are very few things known of the breeding of the ox which are anything else but an illustration of these principles. It is alleged that the famous bull, Shakespeare,* by whom the long-horn breed was so much improved, though of the pure Canley race, scarcely inherited a point of the long-horn type, except his horns; but on examination we find that the genealogy given to prove his purity does not prove it, on the face of it, as all four

* See Youatt, "Cattle," p. 193, edition 1867. Shakespeare was got by Bakewell's long-horn bull D. out of a daughter of the long-horn bull Twopenny, also bred by Bakewell, but the maternal grandmother seems not to be known.

grandparents are not given, and, since he came from a frontier district, at a time when methodical selection had only just begun, there is every likelihood that he was crossed with the short-horn breed which he resembled.

There remains only to speak of the cattle of South America, derived from Old World stock. It would be very interesting if Spanish American cattle, sheep, etc., were examined and compared by some one well acquainted with the breeds of the Old World. This has never been done, and it is rather gratuitously assumed that all the domestic races of tropical America are derived from the existing races of Spain; though there is no improbability in old Spanish races having survived in America which have died out at home, and though the relations of Spanish America seem to have been very strong with Africa, of whose breeds we know next to nothing.

We can therefore attach little or no importance to mere statements of the existence of fine haired, naked, and other peculiar races of cattle in South America, still less to loose talk like that of "two races of cattle in Brazil, one like European cattle (query, what breed ?), the other peculiar with remarkable horns," and so on. But something more must be said on the now famous niata breed, which Darwin, an eye-witness, speaks of thus :—

"These cattle occasionally occur in Europe, but not as a separate permanent race, *i.e.* (in Europe no one breeds them apart, as Indians and others in South America seem to have done)," they are considered by Rütimeyer nearly allied to the extinct form called by him *Bos trochoceros;* their peculiar formation, which causes them to die off during a drought, closely resembles that of the extinct Indian ruminant Sivatherium. Now here, I think, we may see that the niata race is not a new but an ancient form, extinguished by natural selection in time of drought like the Sivatherium which belongs to a land of droughts, occasionally reviving by reversion, and after some such revival preserved in South America by methodical selection. This may be found to throw light on many instances of odd breeds characteristic of places where they are not likely to be indigenous, and specially on some we shall have to deal with in treating of

THE SHEEP.

Of the origin of any of our domestic races of sheep nothing is known. According to Blyth ("Proceedings of Zoological Society of London," Part viii. p. 62), there are about fourteen races of wild sheep now living; how many there may once have been we do not know. Darwin mentions with a note of admiration that Anderson supposes British sheep to be the descendants of no less than eleven native wild forms, "with a bold defiance of all that is known as to geographical distribution;" but if (as palæontology seems to show) our present fauna is only a feeble excerpt of that of the latter tertiary age,* then there is no reason why there may not just as soon have been ten times that number or more.

Therefore not much need be said in detail. Sheep brought into many tropical countries show a tendency to grow more hair and less wool; this is well explained by Darwin as being only a case of unequal development (hygienic). There are two breeds said by Darwin to have suddenly originated in modern times, one in France, the other in the United States of America. The one is the Mauchamp (or silky-woolled) sheep, raised by M. Graux at Mauchamp, in France, from a Merino ram-lamb born in 1828. Darwin very truly observes that if nothing were known of the origin of this variety, many naturalists (those of the school followed by the author) would have felt certain that the Mauchamp Merino descended from, or had been crossed with, some unknown aboriginal form. But in this case we happen to know on the testimony of A. Sanson ("Bulletin de la Societé d'Anthropologie," vol. iv. p. 267), than whom no one is better acquainted with the domestic animals of France, that this variation appears occasionally wherever Merinos are kept, at Rambouillet and elsewhere, and Graux has only the merit of preserving and fostering it. Knowing this, and knowing also as we appear to (*vide* Low, "Domestic Animals,"* p. 134, *et seq.*; Youatt, "Sheep," p. 145) that the Merino is itself a complex of various fine-woolled breeds of antiquity, the suggestion seems to me almost unavoidable that the silky-woolled Merino represents an aboriginal form.

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* See the late Dr. Falconer's "Palæontological Memoirs," vol. i. p. 245.

Of the other, the ancon, or otter breed, said to have originated in Massachusetts in 1791, we know much less as it is now extinct, and we have apparently only one account of it, from a Colonel Humphreys, written in 1819, which account we have no means of controlling or verifying. I suspect that there is some mistake or exaggeration in the alleged suddenness of the appearance of this breed, and that more time and more selection was really employed than appears. We know nothing of the breed of the sheep among whom the first ancon lamb was born, but it seems (*vide* Youatt) that the sheep of America were in a highly mongrelised state—a condition* extremely favourable to reversion. We have also reason to believe that this form has been observed in Europe, and therefore may be considered as a race of unknown origin (*vide* Isidore Geoffroy St. Hilaire, "Histoire Naturelle").

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* The Spanish sheep we find from Columella were crossed with Italian and with African sheep, and the coloration of modern Merinos has been supposed to bear traces of the black, red, and tawny strains mentioned by modern authors.

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* According to Darwin, "Animals and Plants under Domestication," vol. ii. p. 13, *et seq.*

THE GOAT.

Very little need be said of the goat more than that nothing is known as to the origin of any one of its races or sub-races. They are commonly supposed to be derived from *Capra Œgagrus*, but the direction of the horns is different, and it is not easy to see how this could be influenced by domestication.

THE RABBIT.

Of the origin of domestic rabbits nothing is known. Gervais has pointed out that even the grey domestic rabbit is not quite identical with the common wild. Darwin thinks it would be odd if domestication had no influence in altering the rabbit, but whether this seems odd or not depends on the general theory we adopt. Some of the characteristics of the tame breeds (such as lop ears and albinous whiteness) are no doubt due to the direct influence of close confinement, etc., but other marks, *e.g.*, blackness, are marks of races which we have no reason to believe anything else but permanent and original. Lop ears and certain colours—qy. albinous whiteness—are said to be transmitted rather doubtfully, but others, such as blackness sprinkled with white, in the so-called silver-grey, are transmitted with absolute truth.

We are told that "when variously coloured rabbits are turned out in Europe they generally re-acquire the colouring of the wild animal; there can be no doubt (*sic*) that this does really occur, but we should remember that oddly coloured and conspicuous animals would suffer much from beasts of prey and from being easily shot; this at least was the opinion of a gentleman who tried to stock his woods with a nearly white variety, and when thus destroyed they would in truth be supplanted by (instead of being transformed into) the common rabbit." Silver-greys when living in a warren nearly in a state of nature breed true, but a warren must not be stocked with both silver-greys and the common kind, otherwise in a few years there will be none but common rabbits living, through prepotence in intermixture evidently.

In the Falkland Islands there are feral rabbits of various colours, but "how the rabbits were coloured which were turned out is not known." The Porto Santo rabbit is supposed to be a striking instance of transmutation. In the island of Porto Santo there are wild rabbits much smaller than English rabbits, remarkably wild and active (more like large rats than rabbits), nocturnal to a great extent in their habits, and of exceptional wildness, and which it appears breed not easily with other rabbits. They are differently marked from common rabbits, but here Darwin noticed differences in the same individual, which he attributed to the English climate, but which more likely arose from age or the season of the year. As he says quite truly, if nothing else was known of them they would be universally put down as a good species. But they are supposed by him to be the offspring of escaped rabbits, on the authority of a story in the narrative of Cadamosto, and in local histories of Porto Santo, to the effect that Zarco, the founder of the colony, about 1418, had a female rabbit on board which littered on the voyage, and the mother and young were turned loose on the island, and that from them were descended an innumerable multitude of rabbits. It is difficult to speak seriously of this tale as the foundation for grave scientific conclusions, but Darwin reasons gravely on the details "as the rabbit was taken for food, it is unlikely she should be of any peculiar breed" (what breed of rabbits was most common in Portugal, *circa* 1418), "that the breed was well domesticated was shown by her having littered on board, etc." Pursuing the same line of investigation, one would like to know something about the buck, and the ancestors of both sides.

But, speaking seriously, I must say that the whole story is probably a myth, like similar ones about the Andaman pig, the North American fox, and other such "myths of observation," as Burnet Tylor would call them. Granting that the first colonists turned out rabbits, it is impossible to know that there were none before, nor for the matter of that, that none were introduced afterwards, and granted that the Porto Santo rabbit is the descendant of introduced animals, in the words of Darwin as to the Falkland Islands, "how the rabbits were coloured (or otherwise 'characterised') which were turned out is not known." On referring to "R. Major's Discoveries of Prince Henry the Navigator," the latest and best authority on these Portuguese voyages, I find p. 77 that Zarco "had in a cage a pregnant rabbit . . . which littered during the voyage, and with her young ones was taken to the island. The race increased so rapidly that they consumed everything that was planted by the colonists. *The following year*, after a short absence, they found the rabbit increased to such an extent, spite of all their efforts to destroy them, they produced no sensible diminution in their numbers." There is then on the face of the story no sense in connecting Zarco's cage-rabbit with the plague of wild rabbits which appeared immediately on her landing.

THE PIGEON

is supposed to be the strong point of transmutationists, and we often hear the unmixed descent of domestic pigeons from the rock dove spoken of as a fact. Darwin, who has practical experience in pigeons, says that they breed so true that it was a long time before he could believe that all the difference between them was produced under domestication. He says that the more marked races, had they been found wild, would have had separate genera established for their reception; which is perhaps true, but shows only in what a chaotic state the classification of birds is, nevertheless he thinks the evidence of common descent stronger in this case than in that of any other creature. His grounds are chiefly these six:—

1st. If the principal breeds of domestic pigeon have not descended from one stock (the rock dove, *Columba livia,* comprising three subraces: *C. livia* proper, the European rock dove; *C. intermedia,* the Indian rock dove; and *C. affinis,* the chequered wild pigeon, and one or two others equally slightly different), they must have descended from at least eight or nine aboriginal forms very distinct, otherwise the more distinct domestic races could never have been produced. If so, they must either now exist wild somewhere or be extinct. The former, he says, is so unlikely that it may be put aside;

with this I agree entirely. As to extinction since the historical period, he says it is unlikely, as man has not yet exterminated the common rock dove even on small islands.

2nd. This hypothesis implies that several species have been well domesticated, which experience shows to be unlikely.

3rd. Most of our domestic animals have run wild in different parts of the world, but fancy pigeons have nowhere run wild, though, being transported to all parts of the world, they must among others have been carried to their native home.

4th. If domestic pigeons descend from several wild stocks, we must suppose that primitive man, either intentionally or by accident, picked out a most abnormal set of pigeons, long-necked, short-billed, etc., and that these have since all become extinct, which he says is a double unlikelihood.

5th. All the domestic races breed together, and produce perfectly fertile offspring, which is rarely the case with wild species.

6th. All the domestic races, while exhibiting isolated, odd characters, which are almost of generic value, in all other, or nearly all other points, resemble each other and the rock dove. Darwin also mentions that in all or most breeds there occur birds with the peculiar markings of the rock dove.

As to these points, I have to remark that some of them seem to me of little importance in any case, while others might undoubtedly have their weight, if it were otherwise known that modification, and such particular direction of modification as is required, was a *vera causa*, while they are totally incompetent to prove it. As to the first consideration, Darwin would seem entirely to have forgotten all he ever wrote about our ignorance of the causes of extinction, when he assumes that wild birds, which have become extinct since the historical period, have therefore been exterminated by man. We know extremely little of the causes of the extinction of any forms, but we can, I think, see that the same qualities which may be summed up as a certain unadventurous tameness of disposition (as is shown by Galton),* which adapt an animal for subjection to man, may also render its long continuance in a wild state less likely.

The common rock dove has not been exterminated even on small islands, but then it is one of the wildest and boldest of birds, with difficulty, (according to Brent, "Pigeon Book," p. 14) made really tame. The other breeds of pigeons are less wild, shy, and vigorous, and besides had, in the Old World, the rock dove to contend with in the struggle for life. Being extinct they evidently must have had some disadvantage to struggle against in their native country; it is, therefore, not to be expected that their descendants would become wild in it. I can see nothing unlikely in primitive man having picked out grotesque-looking birds, which were also easily domesticable; it accords with what we know of man, both primitive and cultivated. As these races resemble the rock dove, and each other, in everything except one or two conspicuous points, it need not surprise us that they produce perfectly fertile offspring, or that they all share the attribute of breeding freely in captivity, and being easily tameable.

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* Transactions of Ethnological Society, vol. iii. new series, p. 133.

There remains the fact of their odd differences, coupled with general likeness to the rock dove. Darwin thinks that if these races were derived from a cross, the originals must have been equally or more grotesque. But that does not altogether follow, because we know that half-bred creatures often combine the characters of their parents in very odd ways, especially in matters of proportion, still more if their breeding has been directed by selection, with a view to singularity, *e.g.*, if the prototype of the carrier was a much larger bird, then its long neck and huge bill would have been much more appropriate.

Then as to the individual birds in various breeds marked like the rock dove, either these are crossed or not; if they are, *cadit quæstio*, if not, then on the hypothesis of limited variability, they depend on creatures with similar constitutions being attacked by circumstances in the same way; leaving the cause of this similarity undecided, on the hypothesis of no variation, all the breeds may have contained sub-strains *ab origine* coloured like the rock dove. Darwin maintains and gives instances to show that rock dove markings have a tendency to spring up when different breeds are crossed. But in none of these cases does he appear to know much about the pedigree of the particular birds, but pins his faith on generalities, on the unlikelihood of reversion acting for more than twelve generations, and of these birds, "though highly valued by fanciers," being so recently crossed with the rock dove, or rather, perhaps, with similarly marked sub-strains of their own breed, when evidently we want to know all about the history of the particular strains, and the views of the owners through whose hands they had passed; besides that, in what is said about reversion, there is a confusion between a cross diminishing at each generation, as when a black barb, one of whose parents is white, is paired with a black one of pure pedigree, and a cross not diminishing, as when two black barbs are paired, both of whom had one parent white.*

The rock dove, in its three various sub-races, is the common pigeon of all parts of the world, and therefore likely to cross the fancy breeds accidentally. It is also the handsomest sort, or among the handsomest, at any rate, and therefore the most likely to be crossed with them, in order to impress its colour, as the phrase is. In a number of instances Darwin produced birds coloured like the rock dove by crossing pigeons of various breeds and colours, not known or supposed to have any cross, so that either the colour sprang up spontaneously, or the strains Darwin employed (about which he does not seem to have known much in the way of pedigree) were not so pure as he took them to be. There is not much said in the remark that "white fantails breed perfectly true, and have never been known to show any other colour." If pure bred, no doubt, but the question is of the particular strain employed. It is also true that intermediate forms can be found between the most remote breeds, and that some of the breeds have had their characters intensified by selection at no remote period; but these intermediate breeds, and these less characterised forms on which selection works, are they pure or mixed? That is the question, and it is one to which we can find no answer, and which we therefore answer provisionally, at least, in accordance with the principle of inheritance.

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* It is also quite possible that the domestic races of pigeon are crosses between the rock dove and certain extinct types, the rock dove being one element in every breed.

It may be remarked that it would probably have been thought absurd to suppose that the common, Indian, and chequered rock doves, sprang from separate wild stocks, if it had not been known to be a fact; also that the fantail, a variety of Eastern origin, when blue, usually has a blue rump, showing a cross with the Indian rock dove, and also showing the strength of the principle of inheritance. I cannot, therefore, see that (unproved assumptions apart) the famous example of the pigeon offers any particular support to the theory of evolution.

Sanson has made a study of the breeds of domestic pigeon from an osteological point of view, relying on the characters of the cranium and vertebral column, and finds four clearly differentiated races or species, with a fifth, which he has examined less. These, which frequently run athwart the breeds, so-called, of the fanciers, are :—

1. The type of the English carrier.

2. The Bagadotten carrier of Germany. The runts belong to one or both of these types.

3. The type of the rock dove, including most pouters, also fantails, trumpeters, nuns, swallows, etc.

4. The type of the tumbler, including tumblers and barbs.

5. Another type among pouters distinguished by the number of its vertebræ, but whose craniology is not further known. *See* " La Philosophie Positive Revue," No. X. p. 89, and *prec.* and *seq.*

It is evident that these results place the whole question in a light entirely new, and that should they be confirmed by future investigation, the whole of Darwin's argument as to the breeds of domestic pigeons will fall to the ground.

THE FOWL.

The fowl is a case analogous to that of the pigeon, but, according to Darwin, less forcible. The breeds of this bird are supposed all to have descended from *Gallus Bankiva*, or *Ferrugineus*, which is practically identical with the black-red game-fowl; there is no other extant wild form from which they could have descended. Darwin supposes it, as usual, to be extremely improbable that any wild form should be extinct, though the wild game-fowl only manages to exist by its extreme agility, in which it exceeds any other bird, and which the other breeds do not possess (and which is indispensable, owing to the habit the hen has of announcing by a cluck the laying of an egg), not to mention that if the various races existed in the same area the others would be likely to be exterminated or absorbed by the game-fowl, as happens now in a mixed poultry yard.

In crossing different breeds of different colours, Darwin found a tendency show itself to assume "black-red" plumage, so that, as in the analogous case of pigeons, either the sport arose spontaneously, or there had been at some time a cross with the black-red game, which, considering its beauty, strength, and general "game" qualities, is by no means unlikely. We know that strains of fowl intended for exhibition are frequently doctored in many ways, by surreptitious crossing. The same may be conjectured when black-red plumage appears in so-called pure bred Cochins, Dorkings, Hamburghs, etc.; as to the gold-spangled Hamburghs, which have so strong a tendency to run black-red; some of my readers may, perhaps, remember a controversy, famous some time ago in the poultry world, in which it was maintained, on one side, that all sickle-tailed, gold-spangled Hamburghs were crossed with game. What is said about individual variability, *i.e.*, differences between families of the same breed, would be of weight if we had reason to know that all families of the same breed came from one pair.

THE DUCK.

Little need be said on this head beyond what has been said on the fowl and pigeon. We know nothing of the origin of domestic ducks or their breeds.

About the goose, the turkey, and the guinea-fowl nothing need be said more than that nothing is known as to their origin also. Feral guinea-fowl in the West Indies are more likely derived direct from Africa than from Europe.

THE PEA-FOWL.

Darwin, on the faith of several distinct and definite allegations, supposes that the black-winged pea-fowl, *Pavo nigripennis*, was suddenly produced in England out of the ordinary kind. He quotes five distinct accounts of its having been produced among flocks of the common, or common and white, or pied sort, and adduces the usual considerations of probability against there having been an unknown cross.

He also says, "We may suppose that a breed of dogs (rather, colleys, or fox-hounds, dog as distinguished from wolf is much too vague an expression) had been crossed at some former period with a wolf, but had lost every trace of the wolf-like character, yet that the breed gave birth in five instances in the same country, within no great length of time, to a wolf, perfect in every character; and we must further suppose that in two of the cases the newly produced wolves afterwards spontaneously increased to such an extent as to lead to the extinction of the parent breed!"

I think that if a judicious naturalist was told of such a thing under circumstances which did not allow of its rejection as a simple fiction or mistake, he would treat it as we treat wonderful tales we do not altogether reject—he would make allowance for error, and the natural exaggeration by which a singular incident becomes a marvellous one. He would suppose that the strain of fox-hounds was really crossed with the wolf, and that close examination would probably have detected the cross: he would suppose that this crossed strain produced a very wolf-like animal, but by no means a perfect wolf, and that selection did the rest.

Now it appears to me that something of the kind must have happened, specially when we consider that the pea-fowl is a bird which has never been subjected to the fancier's art, and whose breeds or varieties might therefore till quite recently not have been noted with care. As the sport took place once among common and white, another time among common and pied pea-fowl, it seems likely that the cross might have been introduced through a hen, which, odd as such a mistake is, must have passed as a hen of the ordinary white or pied sort. Strange as this is, would it not seem yet stranger, even to an evolutionist, that a sudden modification should have occurred in five independent instances, of so marked and typical a character, both sexes being equally differentiated from the parent stock and from one another. Specially as Swinhoe found this breed in confinement in Hainan, P.Z.S., 1868, p. 30. Now unless it was taken there from Europe, which no one is likely to suppose, either the same new form must have sprung up independently in England and Eastern Asia, which would be contrary to Darwinian ideas, and upset the whole argument from geographical distribution, or it was imported from Asia here, in which case we know nothing of its origin.

As to gold-fish and hive-bees, we know nothing of the origin of their respective breeds, and perhaps not much about the canary. We do not even know if the different Canary and neighbouring islands have different local races from which perhaps the cage varieties may be descended. The yellow colour of many canaries is, however, simply a case of sub-albinism arising from close confinement.

Darwin promised us a work on variation under nature, a promise which has not as yet been fulfilled, but many have written on this subject in a more or less evolutionist sense, but it does not seem to me that this very obscure subject lends much more support to one hypothesis than to another. We find, firstly, cases of little differentiated local races, but whose differentiation is permanent and easily definable, as, for instance, the North American and European red-fox, or the French, Barbary, and Greek forms of the red-legged partridge; secondly, we find such races as I have just mentioned connected by promiscuous intermediate forms along a frontier, as in the case of the Kalij pheasants. Evidently there is nothing in the first to suggest transmutation, and the instances of the second kind suggest original distinctness and subsequent confluence more strongly than anything else.

Next we have different sub-types of the same form, each predominant in a certain district, but not exclusively or even nearly so, as the different-coloured forms of the North American wolf. Then instances where the whole race is variable, like the common ruff, where it is difficult to find two males coloured exactly alike, in this resembling common mongrel barn-door fowls. Last of all we have instances in which abnormal individuals, chiefly in colour, are found in such small numbers that it seems they must be often born from parents both of the normal form, though anything positive is seldom known about the matter, as, for instance, the black form of fox, both in Europe and North America. Now these latter instances seem to me equally consistent with the explanation of original diversity and subsequent intermixture, which is a known cause, and in nature, as well as under domestication—witness the intermixture of the common and Royston crows, the *Tetrao urogallus* and *Tetrao tetrix,* coupled with reversion, which is also a known cause, as with that of transmutation, which is a cause purely conjectural.

Let us suppose two varieties or strains of horse, say white Arabs and black Suffolks, living on adjoining areas; we should probably see a rim of animals with intermediate characters along the frontier, while if the two breeds arriving from opposite quarters peopled a district together, we should no doubt see a promiscuous mass of mongrels, while if one sort were vastly preponderant over the other, either in numbers or prepotence, we should see a tolerably uniform mass of say white Arabs, with only an occasional black specimen, or one in some other way recalling the Suffolk race. I think there can be no doubt that this is what any breeder would predict under the supposed conditions, and it is very like whatever we see in nature to which the name of natural variation can be applied.

Very often we find a pervading character applicable to all the local races of a certain area, as, for instance, that the West American squirrels are black, and the local race of racoon too, and so on. These facts are very curious, but equally consistent with any hypothesis of the origin of types. We do not see how the direct influence of the conditions of life should make animals black; in fact, we know from most varied experience that they have no such influence. Just as difficult is it to conceive how natural selection could have acted so, and why black forms should have primarily arisen in one place more than another is equally unaccountable. The fact gives no support to one hypothesis more than to another. Some naturalist was supposed to have seen nature in the very act of transmuting forms, when what he did see was simply different coloured strains of the same type in one country, the transmutation being read into the facts by preconceived hypothesis.

An eminent naturalist finding that animals to a certain degree intermediate between horse and tapir existed before there is at present any evidence of the existence of true horses, has pronounced evolution no longer an hypothesis but a proved fact, and has framed a pedigree of the horse. But the genealogy is merely conjectural; there is not the smallest positive evidence of blood-connection between the supposed links; there is not the slightest evidence even alleged of the possibility of such changes as would be necessary to convert a hipparion into a horse, or an orohippus into a hipparion; and if the geological record is as imperfect as we know it to be, still more, as evolutionists must suppose, it is almost useless attempting to rely on negative evidence which has so often deceived us. And it is even not wholly impossible, however unlikely, that some day we may find a true horse contemporary with orohippus; and although every intermediate link may have been in particular instances attempted to be traced, the general aspect of palæontology is, as we know it, not favourable to the origin of marked types by slow gradations. Nothing requires more care than the tracing of supposed intermediate links between types of large generality. In one sense every organic type is intermediate between two or more others, and yet how difficult do naturalists find it to classify say the Ruminantia or Carnivora in a regular series, how much more fossil animals, of whom we have only a limited number of usually imperfect remains of the bony parts only.

In a number of instances we are able to show that differences so slight as to appear likely to be most variable are in reality most permanent; let us take external colour (commonly called fleeting and variable) as an instance. And here let me protest against the common use of the terms albinism and melanism. Albinism, in the only correct use of the word, means a diseased state of the system in which there is a total or partial deficiency of pigment, producing pallor or a sickly white, and in the eyes a red colour (that of the blood). This morbid state is often hereditary, specially among animals kept in close confinement, as mice, rabbits, canary birds, ferrets ; but white animals of normal health and with eyes of the normal colour are not albinoes, nor have they any relation to albinoes (the shade of white is also very distinguishable), though they are often improperly so called, especially when rare. There are often white individuals and strains of some animal form not otherwise distinguishable from the ordinary sort, also black strains, but there is not the slightest evidence of the existence of any disorder called melanism and parallel with albinism.

In no instance of all those which have been collected of the occurrence of exceptional black or white specimens of animals whose normal colour is different, is anything really known about their pedigree. It tells us nothing to know that they came from the same brood as others of ordinary colour, unless we know what was the tint of the parents.

Compare the blue-rumped form of the rock dove, which in domestication has never received a separate name or been considered even as a distinct sub-breed, how wild would have seemed the notion of a separate source for this form if it had not been known to be a fact. The same may be said as to the black-breasted red game with red and white ear-lappets, which marks characterise respectively the Indian and trans-Indian local races of *Gallus ferrugineus.*

The Corsican and Sardinian varieties of moufflon differ chiefly in colour, and if they were known only in domestication it would seem quite absurd to suppose for them two wild stocks. The same is true as to many other local races, as for instance the Java tiger, which differs from the Indian only in having its belly somwhat suffused with yellow, or different European varieties of fox, between which the only recognisable distinction lies in the colour of the tip of the tail. Supposing also the several closely allied wild races of mocking thrush, described by Darwin as existing in the Galapagos, had been domesticated, and suppose also that they had become extinct in a wild state, which with regard to races confined each to one small island, would be surely not surprising, would it not be considered quite absurd to suppose that these forms originated each from a separate wild stock? Supposing these only were known in domestication, how puerile it would have seemed, not only to evolutionists, but also to most naturalists who are not evolutionists, to suppose that they sprang from corresponding wild forms.

These and similar facts ought to make us very careful about assuming supposed modifications produced by domestication without positive evidence. With regard to the breeds of so-called modern origin, we must distinguish between those cases where, as with cattle and horses, the improvement was effected through known individual animals, and where, as in sheep, no such celebrated ancestors are on record. The language used by the breeders themselves has rather tended, unintentionally, to confuse the matter. We read, for instance, Darwin, "Animals and Plants," vol. ii. p. 182, of Bakewell having changed the aspect of the New Leicester sheep, when what is meant is that, by encouraging some family strains in preference to others, he altered the general aspect of the sheep in the country. This arises from breeders erecting the abstraction, a breed, into a kind of fetish, just as systematic naturalists make a fetish of species, instead of descending to the ultimate unit of classification—the Family.

But if with Agassiz we start from animal "nations," it is easy to see how by (in any of these nations) selecting certain families and eliminating all impurities in them and exterminating the rest, we change the whole aspect of the nation, and this may be called, rather loosely, modifying the breed, without there being, in a scientific sense, any real modification in the matter. For instance, we know by the experiments of Hofacker, "Ueber die Eigenschaften," etc., and others, the nearly absolute permanence of colour in the horse, but most breeds contain permanent strains of more than one colour; and if, for instance, the predominant white type of Normandy horse were discouraged and roan strains encouraged, the colour of the breed might, in common parlance, be said to be modified. In the breed for instance of Hereford cattle, we can still detect the two "lines of stock" kept by Tomkins, the original breeder, distinguishable by colour chiefly, and, as usual, of the ultimate origin of those sub-types nothing is known.

It is said that Darwinism undertakes to prove, first, that species can possibly be formed by natural selection; secondly, that if they are so formed, it accounts for a great deal of what we see. It is evidently primarily necessary to prove the first, and we see that nothing can be shown for it except vague hypotheses and supposed probabilities, not unlike some to be found in systems of continental jurisprudence. If in any other branch of science facts are stated, they are understood to have been proved. If, for instance, in chemistry we are told that water is produced from oxygen and hydrogen, we understand that the experiment has been tried by scientific men of known repute, under conditions carefully selected to avoid the possibility of mistake, and continually verified afterwards; so if we hear that various domestic pigeons are descendants of the rock dove, this ought to mean that from a strain of the rock dove of known purity, under conditions carefully excluded from the chance of reversion or casual impurity, these several varieties have been raised, and that the experiment can be repeated by any one who has the means and the patience to try it properly.

If that were done, we should have at least a fact to start with, and on which to build our superstructure. Now we have absolutely none ; and if we grant that species as they commonly stand can be produced by evolution, we are only a very little advanced, for it would be arguing from the less to the greater to think that this threw any light on the more important structural differences which separate genera and so on to natural orders, etc., which can only in a few instances be represented as differing only in degree from the former. But now we have the course of scientific research inverted, and Pelion piled upon Ossa by using first the smallness of the difference between domestic varieties to support a guess that they descended from one pair, and then making that last support a still wilder guess as to the common origin of genera, orders, and classes.

To take a very small instance, the presence of callosities on all four legs of the horse (as distinguished from the other equi), how can we have reason to suppose that that has been evolved; and granting that all the varieties and breeds of domestic horse have one origin, what variation do they offer at all analogous to that? The only answer I have ever seen made to this is, that the differences existing between natural species are produced by natural selection, which can act on all parts of the system indifferently; whereas the differences between domestic breeds are produced by man's selection, which can only single out externally conspicuous characters. But let alone that we want positive evidence, it is difficult to say why callosities should be less likely to be encouraged under domestication than length of bill. Mankind, of course, could not select purely internal characters; but if correlation occupies such an important place in the scheme of organised nature as according to Darwinism it must do, such points must often be correlated with those man has selected, also points grossly conspicuous to man must equally often be correlated with others of importance to the being itself. The whole idea rests on a misconception of what in selection we do. When, *e.g.*, we get a white horse by pairing two white horses, and so in a few generations establish a white sub-breed, we simply abstain from crossing; we show the permanence, not the mutability of types. The only case where a doubt occurs is where we get offspring possessing qualities not possessed by their parents, or possessed in a lesser degree; but from what we know of the effects of reversion, we are likely to think that is the cause, unless we know to the contrary.

Very often we are able to trace particular strains back for some time, as in the case of the two branches of Herefords, which descend from two cows belonging to Tomkins (Low, "Domestic Animals," p. 364). I think there can be no doubt that in proportion as we investigate animal forms, both wild and domestic, so we derive an increased notion of the permanence of organised types. If we begin with the popular notion of species, we come to see that the principal varieties must be reckoned absolutely permanent; then we see that this holds equally of less marked sub-races; further, we perceive that we can only rest in still more finely differentiated strains little, if at all, distinguished from what we generally call families, till at last we are brought to recognise the possibility of an idea which has been suggested, to me, and which seems to me very probable, though not yet proved, namely, that except in points in a large sense hygienic there is no variation whatever; that is to say, the children of A and B are the joint result of the characters first, of their parents in varying degrees (which alone would give rise to infinite shades of difference between human beings); secondly, often of some one or more of their four grandparents, C, D, E, F, also in infinitely varying degrees and proportions; thirdly, sometimes of remoter reversions, the same rule applying to physical and mental characters, the origin of primary types being unknown.

But however this may be—and I do not consider this as anything but a conjecture—I feel sure that it is important to begin at this end; instead of starting from variability and asking how far does inheritance extend, we ought to start from permanence and ask how far does variation extend. Most, by far, of the characters of any living being are directly inherited from one or other parent.* When this is not so, they can usually be traced to a grandparent or some near ancestor; when this cannot be done, the matter can often be explained by more distant reversion; but if experiments were tried with the object of ascertaining whether there was any residuum, and if it was ascertained that there are sometimes new characters which arise without inheritance and without any reasonable chance of reversion, then we should define what these characters are and under what conditions they arise; and when we had done this, we should know what we are talking about when we speak of variation and evolution.

I recommend such experiments to all naturalists, specially evolutionists, if they are anxious to found their hypothesis on facts, not merely on conjectures and legends. I feel convinced myself that if such spontaneous variation does exist, it will be shown to be of the most limited and trifling kind; but what is the truth as to this, time and experiment I hope will show.

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* So even Darwin, "Animals and Plants," vol. ii. p. 57. "Hence we are led to look at inheritance as the rule, and non-inheritance as the anomaly."

If, however, we go further and ask the origin of the primitive and immutable forms of organised life, all we can say is, that we know of no cause adequate to produce them, either out of each other, or out of inorganic matter. We may guess, if we like, and no one has a right to complain of another for guessing, so long as he knows and admits that his guess is neither a scientific discovery nor a scientific hypothesis. If we are to guess at all, I quite agree that some form of evolution is one of the most plausible guesses we can at present make; but I think the analogies on which it rests, though often tempting, may very likely be entirely misinterpreted; and it seems to me, personally, that the best thing we can do is to abstain from conjecture on a subject which cannot be elucidated by known causes, but to acquiesce in ignorance, while abstaining also from the negative presumption of dogmatically asserting that we shall never know the origin of living types.

The hypothesis of evolution is so vague that it may be used to explain anything. Darwin was much impressed by seeing fossil remains of huge Edentata in South America, where smaller Edentata make up a considerable part of the existing fauna, and had the hypothesis of evolution suggested to him; but he might have seen fossil Edentata in North America and Europe, where none exist now, and as they have become extinct, while other contemporary types have survived, evidently there have been influences at work all over the world, the nature of which we cannot in any way fathom, hostile to the preservation of Edentata, and these evidently worked less in South America than elsewhere. Under these circumstances it is not very accordant with the harmony of nature that fresh Edentata should be produced in North America and Europe, and it is certain that if they had been so produced they would soon have been extinguished, while if no Edentata had ever been found out of South America, then it might have been supposed that the only reason was, that being first developed there, they had not had an opportunity of migrating elsewhere.

The various aborted and apparently useless organs, such as the mammæ of the male, the useless wings of the apteryx, and other similar instances, are supposed by evolutionists to be quite absurd and inexplicable, except on their theory; and these things, perhaps more than anything else, serve as a popular argument in its favour. But I find it difficult to understand this, when I consider that if we adopt that version of evolution in which natural selection is supposed to be the sole or principal agent of change, we must suppose that a vast number of apparently useless organs have uses unknown to us, or are correlated with other organs which are useful; it is, then, impossible to prove, and even rash to presume, that the wings of the apteryx answer no purpose. If, on the other hand, we agree with those evolutionists who consider the conditions of life or unknown agencies as the main factors in development, then there is no reason why the same causes should not account for imperfect as well as for perfect structures. If these aborted structures were the only ones in which we could see no use, then the explanation would have some *locus standi*.

Also, it seems to me that the evolutionist argument from embryogeny is more poetical than scientific. I do not see why the life of the individual should in that way be supposed to reflect the life of the race. We see a vast number of animal forms, many of which are very like each other, and their distinctions less pronounced in youth. I cannot see anything particularly surprising in this, or any great argument against the permanence of characters. It may seem easier to the imagination to suppose that a strain, say of wild dogs, which should be born black, and turn grey with the first moult, should now and then, without assignable cause, produce a black puppy which should remain black all its life, than that the same sport should take place in a strain which should be grey from the beginning; but it has yet to be shown that this is more likely than the other to happen in nature. It is said, indeed, by Darwin that the observation of domestic varieties (pigeons), etc., shows that variations are apt not to be transmitted to the young as long as they remain such; but we have seen that the forms from the observation of which this is said are, in all probability, originally distinct.

It must be remembered in comparing the youthful and embryonic stages of certain living forms with the adult state of others, that our tests are very imperfect. The ova of all mammals are, at the earliest stage of growth, indistinguishable, yet they turn into dogs, cattle, human beings, whales, etc. Now when two objects, apparently indistinguishable, yet under the same circumstances behave differently, there must be some cause for this; if we believe in a specific vital force as the cause of organisation, then we may perfectly suppose that the ova of man, cow, and dog are materially identical, but if we reject the conception of vitality as something *sui generis*, then we are shut up to believe that these ova, though with our present tests indistinguishable, are really in some way physically different. If it is not one way, it must be the other; and for my present purpose it is quite indifferent which of these two views is the truth.

So the stripes of the lion whelp resemble to apparent identity those of the adult tiger;

but the fact that in the one case they soon disappear, in the other they remain till death, shows that they depend on somewhat different conditions; and we must know much more than we now know (for we now know nothing) about what these conditions are, before we can say whether it would be easier for natural selection, or any other modifying cause, to evolve a striped feline from a lion with stripes on its young, than from some similar creature with no stripes at any time.

It has been said that evolution offers the only or best explanation of the deep-seated and pervading affinities between organised beings on which a natural system is based. It is said that birth from one stock is the only known cause of resemblance between living beings, and that affinity by blood is what is really, though unconsciously, sought for by all who have attempted to construct a natural system. In the first place, there seems to be in the way of putting this a mistake, arising from a confusion between the methods of physical science and those of historico-critical investigation. Science seeks mainly for laws, history for events. If the common descent of all the species of one genus be a fact, this fact will be the cause of the pervading likeness between those forms which causes them to be rightly placed in the same genus, but the justification for their being so placed lies in the likeness itself, whatever opinion we form as to its cause. The distinction between likenesses which imply systematic affinity and those which, however conspicuous they may be, indicate nothing of the kind, is that the one sort are indications of resemblance of kind (see "Mill's Logic"), that is, of a profound principle of likeness pervading the whole structure of the creature, as opposed to being quite isolated likenesses, or associated with nothing except what may be directly correlated with them. The real question is whether we have any good reason to think that common descent is the cause of this pervading likeness.

And here I think we come upon the real gist of the evolution hypothesis, upon that which gives it, to so many minds, both an imaginative charm and an appearance of scientific verisimilitude, and which causes such very doubtful and exceptionable evidence to be eagerly accepted if it only appears to support it. Yet, however seductive this tendency is, however much it takes hold of eminent scientific minds, I think it may be shown to belong to the sphere of mythology rather than of science. It is a common mythological fallacy to suppose that where we see, or think we see, a similarity between two objects, there we may suppose community of origin. It is a portion of the larger fallacy of supposing that things which are associated together in our imagination, are therefore associated in nature. One of the grossest instances of this is to be found in the numerous legends among different nations, in which rocks bearing some likeness to human beings and brutes are supposed to be really transformed men, women, and animals. Another example more to the present purpose may be found in the theories of the alchemists, who, rightly recognising the fact that the metals form a natural class connected together by affinities of kind, were by that led to the belief that they were produced and producible the one from the other.

The crystals form a class very closely allied to the organised creation, a class too divided into many natural subdivisions; but the reason for the similarity of different crystals is not the generation of one out of the other, but the genesis of all of them out of analogous materials under identical laws. If crystals had been known distinctly in the time of the alchemists, there can be no doubt that they would have been supposed by them to have been not as they are, separate and incommutable though analogous formations, but commutable modifications of one primitive crystal, as the metals were actually supposed to be manifestations of one metal, and the salts (so far as they were then known) of one salt. This supposed notion as to crystals, the nearest approach to organism in the inorganic world, would have been strictly analogous to the hypothesis of evolution as now maintained with regard to organised beings. It is no doubt true, as Darwin says, that it is hardly possible to state the facts of homology without using evolutionist language. But one of the most common sources of mythology has been the taking literally the necessary metaphors of language.

Very instructive in this context are the remarks of the late G. Lewes on evolution in "The Physical Basis of Mind," pp. 111, *et seq.* He was himself an evolutionist, yet his line of argument seems largely to sap the deepest foundations of the evolution hypothesis. He seems to consider that the similarities between organised beings have a twofold origin—common descent and community of component elements; and he dwells strongly and forcibly on the improbability that life appeared only in one form and on one favoured spot. If, as strict Darwinians seem to suppose, all organised creatures spring from one cell, or from a number of fac-simile cells, in one small area, then that is totally unlike anything else we see in nature, where nothing appears to be in that way isolated. Everything is a specimen of a class. He says (op. cit., p. 121): "The conclusion seems inevitable that wherever and whenever the state of things permitted that peculiar combination of elements known as organised substance, there and then a centre was established, life had a root. From roots closely resembling each other in all essential characters, but all more or less different, there have been developed the various stems of the great tree."

He also says (op. cit., p. 118): "The absolute identity of chalk in Australia and in Europe is a proof that there was absolute identity in the formative conditions and the constituent elements, but no proof whatever that the two substances were formerly connected by genesis. In like manner the similarity of a plant or animal in Africa and Europe may be due to a common kinship, but it may also be due to a common history. It is indeed barely conceivable that the history, from first to last, would ever be so rigorously identical in two parts of the globe as to produce complex identical forms in both; because any diversity either in structure or external conditions may be the starting point of a wide diversity in subsequent development, and the case of organic combinations is so far unlike the inorganic that while only one form is possible to the latter (chalk is either formed or not formed), many forms are possible to organic elements owing to the complexity and indefiniteness of organic composition. But though forms so allied as those of species are not readily assignable to an identical history in different parts of the globe, it is not only conceivable, but is eminently probable, that orders and classes have no nearer link of relationship than is implied in their community of organised substance and their common history. . . . When we find the metal tin in Prussia and Cornwall and nowhere else in Europe, must we not conclude that in those two countries, and nowhere else, a peculiar conjunction of conditions caused this peculiar evolution."

These remarks appear to me to be true and irrefragable; but if we accept such views, what becomes of the argument for evolution from geographical distribution, the argument from ontogeny, or still more what becomes of the profound assumption that resemblance between organised beings necessarily or probably implies community of descent. Lewes's combination of evolutionist and anti-evolutionist views may possibly be the truth. I see no particular improbability in it, but I do not see how it could ever be demonstrated, or even raised above the position of a mere conjecture, unless we could actually witness the making of organised beings.

Also it is necessarily supposed, not indeed by all evolutionists, but by all pure Darwinians, that organised beings have their structure determined by no strictly internal principle, but only by an indefinite tendency to vary in all directions, taken advantage of by natural selection, giving permanence and gradual increase to such modifications as are favourable to the creature in the struggle for life; but the naturalists who urge this are usually the same who insist very strongly on the analogy between living beings and crystals, in which analogy they seem, to themselves, to see the key to the secret of physical life at any rate. Now crystals develop their symmetrical and typical structure under the influence of strictly internal laws, grounded in the nature and chemical composition of each particular crystal, and with which, as they are not propagated by generation, natural selection, and the survival of the fittest, cannot conceivably have anything to do.

It would seem then, that, according to Darwin, whereas the different species of crystals develop themselves in symmetrical forms, each of most stiff and unyielding type, organisms which are now very generally supposed to be, as it were, crystals raised to a higher power, are evolved from formless protoplasm; or from a single cell with no fixed or internal principle of development by the mere influence of, 1st, a tendency to vary in all directions, 2ndly, the opposite tendency to remain in the groove once impressed, 3rdly, the influence of natural selection. It appears, therefore, to me that the analogy of crystals points much more to the fixity than to the mutability of organised beings, and especially tells dead against pure Darwinism.

From this point of view I join issue *in toto* with Kovalewski ("Osteology of the Hyopotamida," Philo. Trans., 1873, pp. 44, 45), whose remarks are as follows:—"The extreme constancy in the relations of the bones in all paradigitata being ascertained, the problem which is unavoidably presented to the mind of the observer may be stated thus: very irregular small bones, intended to constitute a movable articulation between the long bones of the extremity and the metatarsals and metacarpals, arranged themselves in a certain way in reference to each other, and to these metatarsals and metacarpals this arrangement remains the same in all paridigitata, recent as well as fossil, notwithstanding the greatest diversity of form, size, and habits of life; and if some slight change is to be seen, it is due clearly to the over-development of certain digits, and consequent reduction of others; but in all cases the reason of change is at once apparent.

"How can such similarity in animals so entirely different be explained? To all naturalists who accept the gradual descent and differentiation of all paridigitata from one common form, the fact must appear as a perfectly reasonable and intelligible one. If the immediate progenitor of the paridigitata presented the given arrangement of the tarsal and carpal bones, then, at the gradual differentiation of this type, every small change in one bone called forth a corresponding change in all its neighbours; and as the link that connects all the forms together was never destroyed, and the changes were slowly going on, we meet now, in the extremely differentiated descendants, a unity of organisation which was inevitable, if all these forms descended from one common progenitor.

"But if, leaving the point of view of evolutionists, we look at the matter from the special creation principle, this similarity of structure in animals so widely different is really an awkward fact. To the supporters of special creation, the question presents itself in its simplest form thus: we have now on the earth a large assemblage of paridigitate mammals, presenting widely diversified generic and specific forms, fitted for the most different conditions of life, some leading an amphibious existence, sharing the large streams with crocodiles, while others inhabit inaccessible rocks or burning sand plains; some heavy and sluggish, others light and swift, etc. And yet the creative force, in calling separately into existence these diversified forms, made them all on one plan, and this to such an extent that even the seven bones of the carpus and tarsus, notwithstanding their irregular shape, were always arranged in the same way, so that a certain facet of one bone always touched a certain particular facet of another, and never otherwise.

"That this could really occur in every separate case of creation is almost as probable as that seven dice thrown out of a dice-box should give us the same number of points similarly arranged in a hundred successive throws. Notwithstanding the thousands of different ratios which might exist between seven multangular bones, we get always only one, and in the whole range of living and extinct animals we see no exception to the common rule of typical arrangement of the carpal and tarsal bones. The point at issue is, can this uniformity be accounted for by the principle of special creation, or by the theory of descent and modification? No naturalist can in our time hesitate between the two, and while all the adduced facts are wholly inexplicable by the first theory, they seem most natural in the light of the second. We may still not be fully informed as to all the true causes which induced the variation and consequent differentiation of animal types; but the principle of descent must be conceded as the only one by which all future researches into the structure of the extinct world must be guided."

Here we find, very well expressed, the (as it appears to me) fundamentally erroneous mode of thought of which I am speaking. When he asks why, on the hypothesis of special creation, the carpal and tarsal bones of the paridigitata are all formed on one plan, it is as if any one should ask, why do the other bodies of the universe, as judged by the spectrum and the analysis of meteorites, contain the same elements, *i.e.*, the same combination of sensible properties as this earth does, instead of exhibiting any groups of attributes that the mind can picture? Why do all the planets turn on their axes? Why are the laws of light the same, whether it proceeds from our sun, or from the remotest fixed star which the telescope can reveal? Why do chemical elements combine in definite proportions, instead of in all imaginable? Why does every particle of matter gravitate equally? Why, to sum up, is the world a kosmos and not a chaos?* That this is so renders physical science possible—why this should be so physical science can render no reason. We may either seek an answer elsewhere, in the realm of religion or metaphysics, or we may, with Auguste Comte, pronounce the question unanswerable, and the attempt to solve it simply a waste of the powers of the human mind; but in any case, it cannot surprise us that the universal principle of harmony and order which pervades the whole of nature, also permeates the whole of organic life, and is there most perfectly exemplified. Nor ought we, without positive evidence, to call into account for the harmonious analogies found in animals and plants, special causes like generative descent, natural selection, and the like, which are, from their very nature, inapplicable to the inorganic; yet it is probable that such reasonings as those I have quoted, in spite of their radical fallacy, have done more for the spread of evolutionism than conjectures as to the origin of the fantail pigeon, or legends of the Porto Santo rabbit, and that those last have much rather attained their currency owing to a previous bias in many minds in favour of transcendental evolutionism, than *vice versâ.*

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* And if it should at any future time be proved, as some expect it will, that all forms of matter are modifications of one simple element, the question would only recur with increased emphasis; why does every particle of matter in the universe, at bottom resemble every other particle?

It is often said by evolutionists, if you do not believe that species arose by evolution, how do you think they did arise? Do you think they started out of the ground as they now are? To which the proper answer is—I have simply no theory as to how these races, species, or other, arose; but I think I can show negatively that it was by some process we cannot at present grasp. But do you think we shall ever understand it?

Even that I hold it premature to affirm or deny. As I said before, it seems easier to imagine that a horse was evolved out of an ass, than that it was produced in any other way, specially if we disguise the real incomprehensibility by multiplying imaginary intermediate links; but it does not follow that it is really easier in nature.

It would seem to the eye of a layman (and with regard to the origin of life we are all laymen) easier to make one salt out of another salt closely resembling it, than out of an acid and a base which do not resemble it at all. Yet we know that the one is possible, and the other supremely impossible. So to a savage it would no doubt seem easier to turn, say, a watch on the Swiss horizontal principle into an English chronometer, than to make the last anew out of bare metals; yet in point of fact the Swiss watch, as such, could not be converted, but would have to be entirely reconstructed. Organised beings, whatever else they are, are certainly machines far more wonderful and complicated than any made by human art, and towards these machines we stand related as a savage does to the manufactured products of civilised ingenuity.

The "doctrine of special creation" has been sometimes opposed to that of evolution in such a way as to imply that whoever is not an evolutionist has no choice but to believe that organic forms originated not only apart, *i.e.*, not by generation one from the other, but also without any physical link between them, so that their resemblance cannot be physically explained in any way. But it is not more likely that organised productions, springing up separately, should not show traces of having been formed under the influence of the same general laws than that crystals should not.

Let us for instance suppose that, organised beings are merely chemical products of excessive complexity — of complexity proportioned to their elevation as organisms, and whose systematic affinity is dependent on the resemblance between their elements, and that the excessive rarity of their appearance *de novo* on this planet arises from the complexity and consequent rarity of the collocations of matter necessary to produce them; this hypothesis, though totally without positive evidence to support it—and I certainly do not mean to propose or defend it, is in itself quite as clear and definite, and (what is called) explains the facts about as well as the hypothesis of evolution.

To me it seems that the reason why the hypothesis of evolution appears to many so much more easily conceivable than any other mode of origination, arises mainly from utterly vague and inadequate notions as to the strictness of inheritance, which vagueness is itself the result of forming our notions mainly from highly mixed races, such as civilised man and domestic animals.

Eminent German Darwinians, or rather hyper-Darwinians, have said that the hypothesis of evolution in its strictly Darwinian form is no more to be considered an hypothesis than the law of gravitation ; and we are told that the one proves itself true by explaining the facts, as much as the other. But here the fallacy lies in the word explain. Newton went through a number of calculations stupendously vast, complicated, and exact, and found that these calculations came exactly right when compared with the facts of nature. If this had not been possible, and had not been done, Newton's labours would have gone to limbo with those of many others who united much crude and semi-mythological speculation with more or less prevision of, in a very general sense, the kind of views which science was destined to establish a long time after they were dead.

For whether we take strict Darwinism or any other form of evolutionism, we shall still find it impossible to reduce the hypothesis to a form definite enough to be capable of verification. First take the former.

Organised beings are supposed to have a tendency to vary from unknown causes in unknown directions. This process may go on so fast as to produce a form considered by competent naturalists a good species in a few centuries (Porto Santo rabbit), or even in a few years (black-winged pea-fowl), or so slowly that the amphioxus has not yet, through millions of ages, entirely acquired all the characteristics of a vertebrate animal. The same strain may be benefited by developing in opposite directions, under slightly different circumstances, the difference being imperceptible with any means of observation that we can employ. The modifications go on imperceptibly, and the geological record is so imperfect, that no reliable indications can be gathered from it. Unlimited time is at our disposal, and the way in which changes may be beneficial is practically equally unlimited.

It is plain that such a view, resting on a cause of which we neither *à priori* know the nature, nor inductively can calculate any laws, enables us to take advantage of any facts that seem favourable, and to ignore all of a contrary tenour. If Newton could have supposed the centripetal and centrifugal forces to proceed at uncertain rates, and to counteract each other indefinitely, he would have saved himself a life of labour, but also have ranked as a physicist with Fludd or Campanella. And so here there is nothing which can be required of a Darwinian. There is nothing of which it can be said, if Darwinism is true this must be, the hypothesis is too elastic. But still, one or two remarks may be made by way of showing the reason why I, personally, think that whatever else is the origin of species, natural selection certainly is not. It does not seem to me that natural selection will explain how the higher races were evolved out of the lower. Natural selection tends to make races improve only in the sense of such modifications being preserved as tend to better adaptation to the circumstances under which they are placed.

It is not that this implies greater complexity of organs, or what we generally mean by higher development. I think existing facts point rather to the idea that the lowest forms are those which best fill their places in nature. It does not appear that it would be easier to extirpate vermes than frogs, or frogs than mammals. It seems, on the contrary, that insects and fish swarm as higher beings never do. The way in which natural selection would be most likely to act would be by conferring fertility, hardiness, and early maturity—fertility to make up for losses by enemies or disease; hardiness to enable the creature to live on little food, or various kinds of food, and resist climate; early maturity to minimise the waste of energy produced by the mother having to cater for and protect her helpless young.

Now, it cannot be maintained that in these points the higher animals exhibit any superiority over the lower. In fertility they are immeasurably inferior; in hardiness, probably, on the whole inferior; enormously inferior in earliness of maturity. Now, hardiness and fertility and (though in a very much less degree) early maturity, are points in which closely allied forms differ much, *e.g.*, the hare and rabbit, or different families of the same domestic breed of any animal.* Therefore, on evolutionist principles, it would have been, so to say, an easy task for nature, and one which would have taken a comparatively short time (the importance of this last point will appear later on) to have brought all forms in existence, say toward the middle of the secondary period, up to the highest possible standard of efficiency, in these respects, preserving the general type, rather than to have invented so many wonderful modifications, only indirectly affecting the preservation of individual and race. Instead of which, we find in these all-important points not progression, but retrogression—precisely the opposite course to what natural selection would pursue.

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* These characters are also of such a nature that they would remain beneficial to the creature under any conceivable change of conditions, and so not be liable to the difficulty urged at pp. 135, 136.

In the highest, therefore, according to Darwin, the latest developed of brutes, the anthropoid apes, we find considerably lower fertility than in most other animals, a longer period of infancy compared with the whole life of the creature, and a most delicate constitution ; and it cannot be said that an animal inhabiting the tropics has no need of a constitution formed to resist cold, for in all countries there are relatively cold seasons, which would press hard upon a chimpanzee or an orang. So that these high primates must have lost instead of gaining (as far as the struggle for existence is concerned) in the course of their development out of lower types.

I think the only reason why these facts have not hitherto received as much attention as is their due, is from the want of sufficient distinction between "advance," "improvement," in the sense of "increased adaptation to the conditions of life, in the respects important to the continuance of the race," and the same words in the sense of "increased complexity of organs." An hypothesis of retrogression would probably be more consistent with natural selection than one of progress, but would land its adherents, if it had any, in still more hopeless difficulties, both by the impossibility of finding any one standard of perfection from which all forms could have degenerated, and from collision with the palæontological and ontogenical evidence.

In one most important part of the animal kingdom, however, a consistent evolutionist will be forced by ontogeny to assume retrogression where progress has hitherto been assumed. I allude to the anthropoid apes, whose young are much more like man than the adults are afterwards, and we must therefore suppose, unless we give up the idea hitherto maintained by nearly all evolutionists, and which is one of the bulwarks of the development theory, that the growth of the individual pictures in miniature the growth of the race, we must suppose that these are degenerated from a human or semi-human condition, while the real series of missing links between man and the lower monkeys has been hopelessly lost, which, though a necessary consequence of the theory, makes wider still a gap already quite wide enough.

In addition to the many other considerations which put it into the power of Darwinians to accommodate their hypothesis to almost any conceivable state of things there is this primary reason, that almost every characteristic of an organised being can be plausibly represented as beneficial or injurious to it, or may serve a turn on different grounds, as, for instance, large size is an advantage to one individual or family strain of a given race, in competition for food for females, etc.; on the other hand, small size is an advantage to a race, by enabling a greater number to be supported on the same area, thus making the chances of extermination by accidents less, and on Darwinian principles increasing the chance of favourable variations arising. So, whereas we usually think conspicuous colour must be disadvantageous to creatures liable to the attacks of beasts of prey, it is suggested by Wallace that the stripes of the zebra may be beneficial to it, by enabling individuals the sooner to recognise and rejoin their companions. If so, it would seem that the "desert colour" must be injurious to the wild asses who exhibit it; though it is generally supposed to have been given, or acquired, as a protection against wild beasts. So, in case we had reason to suppose that the earliest animals were omnivorous, it could easily be explained how carnivorous and herbivorous animals were evolved; for, on the principle of division of labour, it is obvious that a stomach adapted to digest either flesh or vegetables, must do its work less well than one of a more exclusive character; while if, on the other hand, primitive animals appeared to be either purely carnivorous, or purely herbivorous, why, then, the matter would be still plainer. What an advantage would any strain of beasts have who developed capacity for a more varied diet. What additional resources they would have in time of casual scarcity. How much better their chances of survival, and how wedded must any one be to obsolete prejudices, who refused to see a truth so obvious.

So the rudimentary organs, on which so much stress is laid, as traces of former perfect organs, could probably be about as well interpreted as indicating the stages by which the organs were developed, and the genealogy of the horse be read in an inverted sense, were Equus Eocene and Orohippus modern. How far from the precision of a scientific law, or even a scientific hypothesis, is a theory which furnishes such easy explanations of almost any conceivable state of facts, however opposite!

Also it ought to be considered that while endeavours have often been made by Darwinists to trace the mode by which the larger divisions have been evolved one from the other, pedigrees have been drawn up of the descent of mammals on the one hand, and birds on the other, from reptiles as a common root, and this has been done with some plausibility, though no one, I should think, can fail to see how remote this is from anything that can be even supposed to rest on positive evidence; yet, on the other hand, when they attempt to account for the origin of the differences which divide closely allied species, while, no doubt, the thing conjectured is less startling to the imagination, and is nearer to what is commonly supposed to be a known fact, the origin of domestic breeds from one stock, yet, at the same time, it is found simply impossible to give any even plausible theory of the mode of development on the doctrine of natural selection.

Take, for instance, the Fringillidæ or the Columbæ. What we see is a number of forms, closely similar in general organisation and type, differing in a great number of minor points, apparently unconnected with each other, or with any teleological purpose these forms existing on the same area, with much the same habits, food, etc. I think it is impossible to overrate the enormous difficulties this state of things offers in the way of pure

Darwinism. I do not mean to say that it can be proved to be impossible that all these types have been formed by natural selection. We know so little of the mutual interdependence of the parts of any organism, and of their relation to the creature's surroundings, that we cannot say of any one character that it cannot have been acquired by natural selection.

But I think it cannot reasonably be denied that the impression produced by the facies of any genus or family is as unlike as anything can be to what natural selection might be expected to produce, unaided by some far profounder and more penetrating cause of likeness and unlikeness. What we should expect unaided natural selection to show us would be a multitude of forms differing in nothing but some one or two points, often or generally traceable to the creature's habitat and the conditions of life under which it exists. Anything more opposite to what we actually do find cannot well be imagined; and this unlikeness makes pure Darwinism, to say the least, extremely unplausible, and as a certain plausibility is the only ground for attaching importance to the evolution hypothesis in any form, this is sufficient for the condemnation of the theory of natural selection pure and simple, which, possessing a kind of apparent likelihood, as long as we keep in the regions of the purely vague, as the evolution of mammals from reptiles, yet is seen to break down utterly when we attempt to apply it to such more concrete problems, as the development of the horse, ass, and zebra from one common origin. On any other form of evolutionism these particular remarks have, of course, no bearing whatever.

Also it may be asked with truth : Darwinism rests on the supposition that all the characters of an organised being are acquired by natural selection, or are correlated with attributes so acquired ; but these correlations, whence do they come? How come indifferent, or sometimes noxious, characters to be correlated with others that are useful? How came it about that animals have to pass through a long period of infancy? Would not the tendency of natural selection be to attenuate this time of weakness and helplessness to nothing? whereas in man we find it longer than in any of the lower animals.

Also, after all that has been said by Darwinians, it remains an enormous difficulty that nothing, or next to nothing, has been discovered which can be interpreted as a vestige of the innumerable fine transitional links which must have existed between species of the same genus, and genera of the same family.

Connecting links are supposed to have been found between the horse and the tapir, and between Canidæ and Insectivora. In such discoveries there is evidently great room for conjecture and subjective bias.

But of the way in which *e.g.* the various Equi proper were developed from one root, if evolutionism be true, we know as little as ever, and I think that what Darwin has said against this must strike a candid reader as a mere plea in extenuation rather than anything else.

There are, besides, some particular characters for which it is difficult to account. Take, for instance, the instinct or principle of attachment to man, which we find in some animals. When this is seen in long-domesticated animals like the dog, and some individual cats, or the horse, it may easily be supposed to have been gradually developed by methodical selection; but we have fairly good evidence of this attachment being shown in a considerable degree by wolves taken young from the forest and tamed (see Griffith's "Cuvier," vol. ii. p. 342, *et seq.*; Lloyd, "Scandinavian Adventures," vol. i. p. 460, *et seq.*; Lloyd, "Northern Field Sports," vol. i. p. 345, *et seq.*). It is understood to be conspicuously manifested by the elephant, an animal which never breeds in confinement, and every individual of which is forest born; and this same principle of loyal attachment to a human master is also very strongly marked in the bullfinch, a bird which breeds scarcely at all in confinement. I have known a bullfinch which refused to eat on being separated from its mistress; while on the other hand, the canary, though of the same family, now long domesticated, shows little or no attachment to man whatever. Now how is it conceivable that wild animals should acquire by natural selection the tendency to love man? Of what use could it be to them in a state of nature? *

It also seems to me that there is another point which has not been enough remembered. The whole hypothesis goes on the idea that we have practically unlimited time to dispose of.

Lately arguments, whose force (from ignorance of the subjects dealt with) I am entirely unable

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* See Note, p. 173.

to estimate, have been brought forward to show that the time occupied by the past history of organic life, though vast, is still confined within definite and ascertainable limits. But whether this be so or not, I think it is not remembered that when we speak of natural selection gradually by insensible modification producing characters advantageous to the race under particular circumstances, we ought to consider the likelihood that before the organism is vastly changed the circumstances may be changed too, whether the circumstances be cold, heat, moisture or the presence of noxious, or food-producing other organic forms, etc. It will be a race between the permanence of a given type and that of its surroundings, and when we consider that within the period since we can be sure of the existence of the present varieties of man (the polished stone period, probably about 8,000 or 10,000 years ago at least), no organic form seems as far as we know to have changed in a state of nature, while changes of the land- and sea- line, the extinction, local or entire, of many forms, all changes connected with the spread of man, must have greatly altered the surroundings of every form now existing. Considering this, it seems likely that the type would prove more obstinate than its external conditions.

It may also be not unfairly argued that Darwin's theory of sexual selection is not very consistent with his general principles. In the first place, it is said that (bright or conspicuous) "colour being dangerous, ought not to exist in nature," and there is no doubt that, *cæteris paribus*, dull colour is an advantage in harmless animals, by concealing them from their natural enemies—in animals of prey, by hiding their approaches from their victims; to which it has been answered that only those forms are bright-coloured which have some other means of protection. But granting this, still, on Darwinian principles, strains which were dull-coloured would have an advantage, and would gradually supplant the others, there being no reason why they should have less of any other protection.

Here comes in the theory of sexual selection by which it is supposed that female animals, especially birds, prefer the brightest coloured males, who consequently when (having some other protection) they are not absolutely stopped by natural selection, have more progeny than others, etc. We shall, I think, see later that there is no evidence for this preference of females; but if it were a fact, it could be no more permanent or immutable than anything else in the animal constitution, mental or physical; it must have been acquired, and acquired too in the teeth of natural selection, because those strains of animals whose females had a taste for less gaudy and more useful coloured mates, would gradually supplant those families whose females had more frivolous tastes, till in course of time dull colouring and the preference for it would be established throughout the animal kingdom.

But there are many evolutionists who are not strict Darwinians, none of whom have attempted to give their views the same systematic distinctness as Darwin has to his. But before considering this subject it will be well to say a few more words on sexual selection, by which the lacunæ in the hypothesis of natural selection are attempted to be filled up. The hypothesis rests on these alleged facts :—

That the law of battle prevails extensively in nature; also (which last is what is principally meant by sexual selection) that female animals exercise an extensive power of choice among males; that they select such as exhibit slight variations in plumage, etc., which happen to be attractive to them, who therefore have a better chance of leaving offspring than others, and that this is the origin of the complicated and brilliant colours of animals. On this it may be said, as I have already pointed out, these preferences on the part of female animals extending in the same direction from generation to generation, supposing them to be a fact, require to be accounted for like any other characteristic of animal races, and they would seem to run in the teeth of natural selection, which would have developed females with a taste for subfusc and safe coloured males.

Also it is involved in all the nebulous vagueness of Darwinism generally. If male animals are decorated with colours to us beautiful, evidently this is the result of selection by successive generations of females. If the colours are not beautiful to us, who knows what kind of taste the female animals may have? If the sexes are coloured almost uniformly, why then characters originally belonging to the male only have been transmitted to both sexes, whereas in other closely allied forms, as near, for instance, as the common to the green-necked peacock, they are transmitted to males only without our being able to assign any reason for the difference. Birds, for instance, are much more beautifully coloured and more sexually different than mammals ; have female mammals less taste or power of choice than female birds ?

But the great objection to sexual selection is total want of evidence for this supposed preference of female birds for abnormally coloured males. This evidence is attempted to be supplied by various anecdotes, which show certainly that female animals often exercise choice, but do not at all show that their choice is regulated by any principles corresponding to what we call æsthetic. Nor does it well agree with what we know of the psychology of brutes to suppose that it is. I can see no reason to think that modifications in the colour of animals considerable enough to be noticed would be agreeable to the females, but rather the reverse.

Some traces of a love of bright colours may perhaps be found among some animals, but the rule I think is contrary, and we know that creatures at all different in appearance from their compeers are almost certain to be baited. The whole notion of gay colours in males being attractive to female animals is derived, I think, from a most unsafe source, the anthropopathic interpretation of the instinctive actions of animals, of those actions which all animals of the same race-type perform in the same manner, such as their gestures in courting the female.

It is necessary, however, to take notice of Grant Allen's attempt to account for sexual selection. His theory is that females learn to prefer bright colours when they are accustomed to feed upon bright-coloured fruits and seeds, and he attempts to show this in detail, though, as it appears to me, with very considerable partiality. The thing which requires to be accounted for (letting alone that sexual selection is quite unproved) is not simply the presence of bright, but the presence of various different bright colours, arranged often in beautifully symmetrical patterns, separately characteristic of different living forms.

It is true that the most conspicuously beautiful birds, as, for instance, humming-birds and sun-birds, are fruit-eating; but Grant Allen does not attempt to point out any particular relation between their colours and that of the fruits they feed on, though in the Colour Sense (p. 185) he insinuates that this might be done. Yet it is evident that till such a test is applied nothing has been really done for the support of his hypothesis. If the colouration, say of birds, is determined by the predilection of the females, then female birds have an eye for minute shades of difference amid exquisite arrangements of colours, and of different strains of female birds each has retained and improved through countless ages its own pet colour and pet arrangement. If these predilections of female birds are to be accounted for by their food, then birds with much blue in their plumage must feed on blue fruits, those with bright yellow on yellow fruits, and how the arrangement of colours is to be accounted for on that principle I cannot guess, nor has Allen given any hint. Even if we find a certain plausibility in Allen's hypothesis, as long as we limit ourselves to divisions of the largest generality, how is it to be applied to closely allied species of the same genus with similar habits? Allen, for instance, singles out the bullfinch as the most notable of our brighter-coloured finches, and as specially fruit-eating; but is the bullfinch really brighter than the goldfinch or chaffinch? Again, why are grass-eating mammalia never green? According to Allen's theory one would think cattle and sheep ought to be predominantly so. Is it answered that green, through its commonness or any other reason, does not impress the eye of female animals? Then why is bright green one of the commonest elements in the colouration of the more brilliant birds? As applied to mammals, this theory breaks down still more completely, and Allen is obliged to strain his instances very much. For instance, the monkeys, though affording some brightly coloured forms, are in general a particularly dark or subfusc family. The squirrels, again, offer no particular support to his theory, their colouration offering a remarkable analogy to that of the canines. Seriously, who can contemplate the systematic colour of the spotted and striped felines or viverrines, or the systematic and delicate though not bright markings of so many birds—the sea-gulls, for instance—and think that they stand in any relation to their food? And the markings of the bright-coloured birds stand in sufficient relation to these to make us sure that the phenomena are of like kind. Grant Allen accounts for the colour of flowers in a way which it does not lie within the province of this work to criticize, yet even he is compelled to admit that the brilliant colouration of such large classes, as sea-slugs, sea anemones, etc., cannot thus be accounted for. On the whole, I consider that Allen has failed to make out his case with any probability, and that there is no reason to think he has removed any of the difficulties attaching to sexual selection.

To continue the subject, none of the evolutionists who are not Darwinians proper have attempted to give much systematic detail to their views. Sometimes the direct effects of known conditions (temperature, moisture, etc.) have been called in to fill up the deficiency, but these effects are so plainly incapable of producing the complex results we find, that much time need not be spent in discussing them. There is not really any evidence that colour is permanently affected by climatic change; geographical races marked mainly by colour often correspond with geographico-climatic areas; but there is no evidence of a race or a family strain changing its colour through climatic influences; in fact, our most ordinary experience with domestic animals is enough to show us that strains of any and every colour may be kept for any length of time under every sort of climatic conditions without any change. But these writers of whom I am now speaking generally refuse any kind of definition of the conditions of life on which they lay such stress. Witness Dr. Andrew Murray, "Geographical Distribution of Mammals." The only other form I know of the evolution hypothesis is that which supposes types to develop fresh sub-types in a way somewhat analogous to the growth of the organic individual, and to this I can see no particular objection; but neither do I see any particular reason in its favour, and I do not think that much is explained by supposing it without any positive evidence. Somewhat similar views are entertained by Mr. Mivart, "Genesis of Species;" but much of what he says seems (though intended only against Darwinism) really to tell against the hypothesis of evolution in any form, while his instances in support of sudden modifications are mostly doubtful. I allude specially to what Mivart says on independent similarities of structure. He specially instances the sub-divisions of Marsupials corresponding to many of the orders of Placentals (the thylacine to the Carnivores, the kangaroos to the Macroscelides, the wombat to Rodents), which suggested at one time to Huxley the idea that these marsupial sub-orders were the origin from which the placental ones had been developed. If this be true, the characters marking a Placental as distinguished from a Marsupial must have been several times independently developed; if we take the more common view, viz., that all Placentals and all

Marsupials came respectively from one immediate stock, then the characters which, *e.g.*, the thylacine has in common with Canis, and so on, must have been independently developed.

A similar case he gives is that of the relations of the Carinate birds (all, except the ostrich family) with the ancient Pterodactyle reptiles on the one hand, and of the ostriches with the ancient Dinosaurian reptiles on the other. Here, again, there is the same alternative. Either birds had two origins, so that the distinctive characters of the class were independently produced, or all birds originated from Pterodactyle reptiles, and the characters common to ostriches and dinosauria originated independently, or all birds sprang from Dinosauria, and the similarities between Carinate birds and Pterodactyle reptiles were separately evolved. These instances Mivart considers "a dilemma, either horn of which bears a threatening appearance to the exclusive supporters of natural selection," because "on this theory the chances are almost indefinitely against the independent accidental preservation of two similar series of minute variations resulting in the independent development of two closely similar forms,"* and he considers that any other form of the evolution hypothesis is not affected by the argument; but it is evident that the mind, when it is once accustomed to consider similarity as arising independently of descent, is likely to have its faith shaken as to the hypothesis in any form.

The same may be said of Mivart's remarks on specific variability. He points out with justice that if species had been formed by gradual transition, more traces must have remained of the intermediate steps, and thinks that this difficulty can be avoided by making the transition sudden, in support of which he gives instances like that of the black-shouldered peacock and others. Now, even if the black-winged peacock is a variation, which is doubtful, it is a very slight one, differing, so far as is known, in colour only. Just in proportion as we make the transitions between organised forms sudden, we remove them from the category of anything that can even be plausibly alleged to be observed.

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* This, however, seems doubtful. See p. 106.

In fact, it may be said that Darwin's best work has yet rendered the mutability of organisms more unlikely than before, for while he has deserved splendidly of science in calling attention to the principle of natural selection, it must not be forgotten that natural selection is a principle which by itself accounts for nothing else than the preservation and destruction of various forms of life; in no way by itself does it account for the origin of new types, except by supposing in addition an indefinite variability, for which there is no evidence. Natural selection is a fact; no one can doubt when his attention has once been called to it, that of slightly different strains of organised life, the one which has any trifling advantage is likely to prevail over the other in the long run; but nothing can be inferred from this, unless it is conjoined with the notion of unlimited variability, while, contrariwise, natural selection being certain, any facts which lead to the opinion that organised forms are not as the unconditional workings of natural selection, coupled with spontaneous variability, would have made them, throw doubt, not on natural selection, but on the variability of living types.

What are the characters which conduce to success in the struggle for life? As I have elsewhere said, hardiness, omnivorousness, and fertility, much more than any complicated special adaptations—witness the rabbit; these characters are also what evolutionists call highly variable, being very different in closely allied forms. Why, then, has natural selection not stamped these characters much more largely than is actually the case? It seems to show that the material was intractable, and that therefore living types were not produced by evolution, or at least not by evolution under existing conditions.

THE BEARING OF GEOGRAPHICAL DISTRIBUTION ON THE ORIGIN OF LIVING TYPES.

THE geographical distribution of animals and plants is generally supposed to lend strong support to the hypothesis of evolution ; and any mind at all impartial must see that it offers a nearer approach to evidence than anything else that can be urged. No one but an advocate can deny that the general aspect of the facts connected with organic geography does tend to suggest the idea that genera, species, etc., originate by descent, even if in some manner the nature of which is now inconceivable; or, to express the matter entirely without hypothesis, it tends to show that a new form of life only springs up where closely resembling forms already exist in the neighbourhood, which of course suggests descent.

It is now pretty generally known that similarity of products between different areas depends more on the presence or absence of communication than on similarity of circumstances, and that organic types are diffused, on the whole, in proportion to the means of transport possessed by them; that the number of forms is small on oceanic islands; that barriers of any kind have an important influence on the facies of the flora or fauna of any country. This is all true, but it has been exaggerated. Difficulties have been slurred over. Then, as I have often said, there will be always found the difference between adventurous and cautious minds. It is well if we remember that conjectures are not science, however much they may be prophecies of what may be science at some future period. Sometimes the facts will not bear investigation. We find, for instance ("Origin of Species," p. 417), that "no two marine faunas are more distinct, with hardly a fish, shell, or crab in common, than those of the eastern and western shores of South and Central America. Yet these great faunas are separated only by the narrow but impassable Isthmus of Panama." Later investigations, however, seem to prove (Wallace, "Geographical Distribution," vol. ii. p. 20) that there are at least 35 species of shells absolutely identical, whilst so many more are so close that they may be only varieties. Nearly 70 others are distinct but representative species. Of the genera of marine mollusca, more than 40 are common to the East and West Coast. Thirty per cent. of known fish are also identical, not to mention the affinities shown by Miocene corals. On these grounds Wallace supposes that the isthmus has been submerged in comparatively recent times. Comment on this is superfluous.

It is simply an instance of over haste; but there are other things to be said which cut deeper into the whole argument.

It has been said, with great *primâ facie* appearance of justice, that the fact that scarcely any mammals are found on remote islands tells strongly in favour of derivation by descent; it does not appear why mammals should not have originated on islands as easily as other creatures. We know that they are well capable of maintaining themselves there wild when once introduced by man, while it is easy to see the special difficulties there are to their introduction in a state of nature. It is, however, remarkable that many islands poor in mammals contain other creatures whose transport would seem to be equally difficult, as, for instance, New Zealand, which contains apparently no single indigenous mammal, contains that remarkable wingless bird the Apteryx, and contained within the recent period the still more remarkable group of Dinornis. Now how was it easier for these struthious birds to be transported than for mammals? Did they come over with wings and then lose them through disuse? But winglessness is characteristic of the whole struthious order. But New Zealand is not properly an oceanic island. It was recently connected with Australia. Then why has it no native mammal instead of being full of marsupials?

Since the time when Darwin wrote that Batrachia were never found on oceanic islands, tree frogs have been identified in the Fiji group (*vide* Wallace, "Geographical Distribution," vol. i. p. 449). The same may be said of the land tortoises of Galapagos. But the most instructive instance in this respect, perhaps, is that of the West Indies. There we have a marked insular fauna, almost no mammals that are indigenous (although cattle and goats have flourished and become feral under European auspices), but an enormous profusion of land shells surely as difficult to transport as mammals. And we know as a positive fact that large mammals of the Chinchilla type existed on one and probably more of the West Indian Islands. Now if the islands were never connected with the mainland, how did the large mammals get there? If they were once continental, how is it that they no longer have a continental fauna? It would seem that there must be some cause tending to extinguish mammals on islands after a certain lapse of time, though they do well enough for a short period. One cause may evidently be the greater risk of extirpation within a small area. We may take it as certain that mammals, from their large size and comparative confinement to one spot of ground, are amongst the most easily extirpated of living things; and, let us ask, if the small islands of the Pacific had once been thickly peopled with mammals, what chance they would have had against the spears of the natives in an area of about ten miles square. I by no means contend that this is the sole cause, or perhaps even the chief cause, of the paucity of mammals on small islands, but, as I have said, I think it proved that there is some cause hostile to their existence in such places.

By this and other considerations we are led to the conclusion that the facts of distribution can, to a great extent, be shown to have originated in an opposite manner, not by the origination of new forms, but by the destruction of old.

On this subject we may see some suggestive remarks in Dr. Andrew Murray's "Geographical Distribution of Mammals," p. 2. On the one hand an important argument for evolution is supposed to be derived from the presence or absence in small islands, etc., of certain large classes of living forms, on the hypothesis that the distribution of land and sea has remained much the same since the beginning of the recent period; on the other hand we find some evolutionists on zoo-geographical grounds, calling up huge continents and arms of the sea otherwise unknown to geologists, thus sapping the foundations of the former argument.

In fact, there is much reason to think that towards the end of the tertiary period, the general facies of the fauna of this planet, or at all events the northern hemisphere, was tolerably similar, every type being cosmopolitan, while each country had its own local races, as is the case now in some groups, say the Canidæ, and that what we see now of certain groups being characteristic of certain tracts is the result of subsequent extinction taking place diversely in different parts of the world. Very often, when judging simply by the existing fauna and going on evolutionist principles, we should be tempted to suppose that special intercourse must have existed once between two countries ; an examination of their fossils will show us that the resemblance between their forms is only that between the two ends of a long chain, the (geographically) intermediate links of which have become extinct.

For instance, it has been supposed that there was once a connection between South America and the Indian Archipelago, and the existence of races of Tapir in the two has been supposed to be evidence of this ; but this argument falls to the ground when we remember that forms of the same type in the tertiary age ranged over North America and Europe. Why types should have become extinct in one place and not in another sometimes we can tell, but very often we cannot, any more than an evolutionist can say why a particular variation is developed in one place rather than in another. The same applies to the argument for connection between South America and South Africa derived from the presence of Edentata in both, an order which belonged in tertiary times also to Europe and North America.

So it is a favourite speculation that there was once a continent connecting South Africa and India, but of the common types on which this is founded many, as for instance the large felines and the large Pachyderms, can be shown to be once cosmopolitan; also the same may be said as to the Edentata in common between South Africa and Brazil.

Some other items of circumstantial evidence, relied upon by evolutionists, seem to be rather doubtful. For instance, Wallace laid much stress upon the supposed fact that whereas there was shallow water from India to Java, and shallow water again from Celebes to Australia, arguing land connection in no very remote past, there was deep sea between, on the one hand, Java and its appendages, and on the other Celebes and its appendages, showing that there had been no land connection for countless ages, and that a very marked division between the Indian and Australian fauna corresponded exactly with this; whereas he has now come to the conclusion that the Celebes fauna partakes about equally of both, which latter conclusion is certainly in accordance with his lists of organised forms in particular.

Also it ought to be remembered that this reasoning is deeply tainted by the vice of indefiniteness, the besetting sin of evolutionist speculation. When a subject is very obscure, the habit of calculating all the slight chances for and against suppositions, which have nothing in their favour which deserves the name of serious evidence, is one quite contrary to the proceedings of science. Nor is it very easy for evolutionists to make up their mind whether they suppose land and sea to have remained pretty constant since the origin of the present forms of life or their nearest ancestors, or whether they suppose them to have greatly changed. If they think the former, they are involved in great difficulties; if to escape these they resort to the latter alternative, they undermine the whole geographical argument for evolution. *E.g.*, if South America and South Africa were *ab initio* perfectly separate, how do we account for the presence in both of atruthious birds? If they were recently connected by land now submerged, then what becomes of the argument from the generally different facies of their faunas?

Again, how is it explained that the mammalian fauna of Australia consists almost entirely of marsupials? To give this any meaning on the hypothesis of evolution, it must be supposed that Australia was separated from Asia ever since the beginning of placental mammalia, *i.e.*, at the very beginning of the tertiary period at least. For marsupials were once dominant over the whole world, therefore no local product of

Australian conditions. Why did they remain in Australia and not elsewhere? The evolutionists can give no answer which does not imply that the deep but extremely narrow sea between Bali and Lombok has remained unchanged since the Eocene period; rather a large assumption, considering what we know of geological change.

It is evident we have to do with laws we do not understand and about which conjecture is rather useless. It is often said that the hypothesis of evolution explains so many facts connected with geographical distribution that we may feel sure that the difficulties will be some day explained; but every mythological hypothesis was founded on some analogy or other, and it appears to me that in this case suspension of judgment is the best.

No one can look at the general facts connected with geographical distribution without the suggestion of evolution entering his mind, but on the other hand, on more minute investigation, he will find that all his general views depend on the assumption that hydrographical relations have been much the same since the origin of existing races or even existing types of greater generality, while at the same time he will find himself compelled in pursuing his notions into detail to suppose hydrographical changes which must be called immense, so that he may perhaps be inclined to feel doubtful of the original suggestion, and to do more than doubt of evolution; to pronounce dogmatically against it I feel as little inclined as any man living.

Speaking generally, it may be said that while large inferences have been drawn as to the origin of species from comparing the present state of different countries with the facies of their fauna, and sometimes from that facies inferences have been drawn as to the former geological relations of land and sea, not nearly enough has been done in tracing these ancient geological relations as far as possible by independent evidence, and comparing the results arrived at with the distribution past and present of organised beings. When this has been done, I believe that it will be evident that though a certain appearance will still be presented as if of evolution, yet this appearance is not one which gains on being pressed into detail, as it ought to gain if it is to be the foundation of a scientific hypothesis; but, on the contrary, that it has something of the nature of the mirage, which when approached recedes further into the distance.

CONCLUDING REMARKS.

THUS we see, after reviewing all that is said for and against the hypothesis of evolution and the special Darwinian form of it in particular, that this hypothesis is itself built on doubtful hypotheses as to variation; that it starts from supposed variations under domestication which are entirely unproved; then, assuming that some animals vary in some directions, it infers that all animals vary in all directions, contrary to the analogies of the rest of nature; that many of the grounds on which it is supported are inspired by the idea that all like things are in fact the same thing transmuted, which idea was at the bottom of alchemy and many other mythical dreams; that the processes by which evolution is supposed to act are so vague and undetermined as to be equally incapable of proof or disproof; that as far as the hypothesis of natural selection pure and simple is concerned, the facies of animated nature is in many respects different from what it would be if that were true, while no other form of evolutionism has ever assumed even an appearance of scientific precision; that the support evolution derives from geographical distribution, not unjustly supposed to be its stronghold, is not so great as is commonly supposed, and does not become more when carefully looked into, but the contrary; that sexual selection is a dream.

From all these things it seems to me we must consider that evolution is not only not a proved law, but not even a scientific hypothesis, but a mere conjecture which may be proved or disproved at some future period, yet likely at present to remain a guess. We must therefore for the present, provisionally at any rate, consider as permanent not only what are called species, but also varieties, sub-breeds, and very likely family strains; while as to how these were originally produced we must be content to own that we know nothing.

NOTE.

THERE are many more improbabilities attaching to the belief that living forms, as we see them, are the unaided products of natural selection, when once we attempt to pursue it into detail. As, for instance, we may ask, and it is not easy to give a good answer to the question, how it can ever happen that an ancestral and an evolved type exist at the same time, while the links between the two have perished? If, for instance, birds have been evolved from reptiles, reptiles from amphibia, and amphibia from fish, why have the intermediate links perished while fish still exist? How come amphibia to coexist with reptiles, if the transitional links between reptiles and amphibia have perished; because, if their intermediate forms were unable to compete with reptiles, why do amphibia remain; specially as the earlier stages of transition must have come more into competition with amphibia than with reptiles? And the same may be said of reptiles and mammals, the anthropoid apes and man, etc. It may be said that our amphibia are not precise representatives of the original amphibia out of which reptiles were evolved; but still, for the present purpose, the type amphibia may be considered as one. If the original amphibia, preserving the type amphibia, became so modified as to hold their ground co-existently with their improved descendants the reptiles, why did this favourable modification not take place in any of the intermediate stages between the two—which were, *ex hypothesi*, an improvement on amphibia, or they would never have been evolved?

Also there is another difficulty, even more formidable, that has been very well put in the "North British Review," vol. xlvi. p. 288, *et seq*. It is that no race could be formed in a state of nature by a sport, however advantageous, unless it be supposed, in addition, quite gratuitously to possess a quite unheard of and almost impossible degree of prepotence; because, otherwise, its advantages would be more than counterbalanced by the numerical superiority of the original form. "A million creatures are born; ten thousand survive to produce offspring. One of the million has twice as good a chance as any other of surviving; but the chances are fifty to one against the gifted individual being one of the hundred survivors. No doubt the chances are twice as great against any one other individual; but this does not prevent their being enormously in favour of *some* average individual," and the favourable variation would soon be reabsorbed into the ordinary form. The Reviewer goes on to say: "An illustration will bring this conception home. Suppose a white man to have been wrecked on an island inhabited by negroes, and to have established himself in friendly relations with a powerful tribe whose customs he has learnt. Suppose him to possess the physical strength, energy, and ability of a dominant white race, and let the food and climate of the island suit his constitution; grant him every advantage which we can conceive a white to possess over the native ; concede that in the struggle for existence his chance of a long life will be much superior to that of the native chiefs ; yet from all these admissions, there does not follow the conclusion that, after a limited or unlimited number of generations, the inhabitants of the island will be white. Our shipwrecked hero would probably become king ; he would kill a great many blacks in the struggle for existence ; he would have a great many wives and children, while many of his subjects would live and die as bachelors ; an insurance company would accept his life at perhaps one-tenth of the premium which they would accept from the most favoured of the negroes. Our white's qualities would certainly tend very much to preserve him to a good old age ; and yet he would not suffice in any number of generations to turn his subjects' descendants white. It may be said that the white colour is not the cause of the superiority. True ; but it may be used simply to bring before the senses the way in which qualities belonging to one individual in a large number must be gradually obliterated. In the first generation there will be some dozens of intelligent young mulattoes, much superior in average intelligence to the negroes. We might suppose the throne for some generations to be occupied by a more or less yellow king ; but can any one believe that the whole island will gradually acquire a white or even a yellow population, or that the islanders would acquire the energy, courage, ingenuity, patience, self-control, endurance, in virtue of which qualities our hero killed so many of their ancestors, and begot so many children—those qualities, in fact, which the struggle for existence would select, if it could select anything?"

These considerations seem to me conclusive, at any rate against the formations of complicated organs, like the eye or the ear, by natural selection; which would seem to imply that exceptional and individual variations not once, but tens or even hundreds of times, have become common to a whole race. It would seem, then, that while there are a vast number of characters (those apparently useless) for which natural selection accounts with great difficulty—others, the complicated organs, for which it cannot account at all—there are only such as increased strength, speed, hardiness, etc., for which it accounts at all well, even granting indefinite variability; and as to these very characters, we find that they are not nearly as widely diffused as we should expect to find supposing animal types to be really indefinitely mutable, closely allied races often differing most remarkably in these very respects (see above, p. 123, *et seq.*). Against transmutation independently of natural selection, this line of argument tells even more strongly than against Darwinism proper; for then, instead of a sport possessing some striking advantages over the parent form, and gradually reabsorbed by force of numbers, we have a sport possessing no particular advantages, and therefore certain to be reabsorbed immediately. All these considerations impress upon us more and more that the origin of organic types is at present, and till we have some new knowledge enabling us to attack the problem in quite a different manner, an insoluble mystery.

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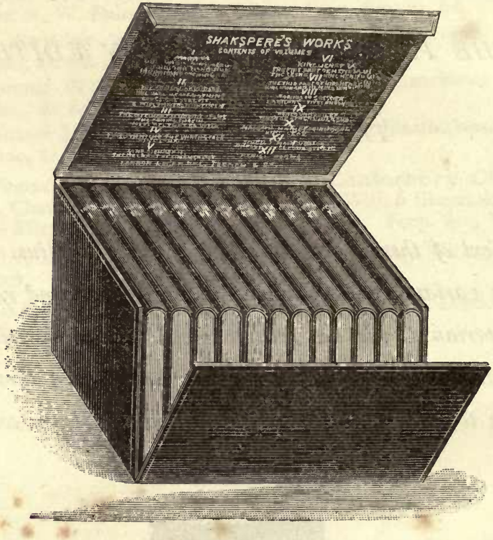

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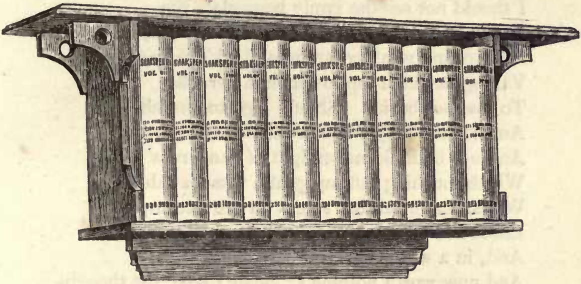

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Salar. My wind, cooling my broth,
Would blow me to an ague, when I thought
What harm a wind too great might do at sea.
I should not see the sandy hour-glass run
But I should think of shallows and of flats,
And see my wealthy Andrew, dock'd in sand,
Vailing her high-top lower than her ribs
To kiss her burial. Should I go to church
And see the holy edifice of stone,
And not bethink me straight of dangerous rocks,
Which touching but my gentle vessel's side,
Would scatter all her spices on the stream,
Enrobe the roaring waters with my silks,
And, in a word, but even now worth this,
And now worth nothing? Shall I have the thought
To think on this, and shall I lack the thought
That such a thing bechanc'd would make me sad?
But tell not me: I know Antonio
Is sad to think upon his merchandise.
Ant. Believe me, no: I thank my fortune for it,
My ventures are not in one bottom trusted,
Nor to one place; nor is my whole estate
Upon the fortune of this present year:
Therefore my merchandise makes me not sad.
Salar. Why, then you are in love.
Ant. Fie, fie!
Salar. Not in love neither? Then let us say you are sad,
Because you are not merry; and 'twere as easy
For you to laugh, and leap, and say you are merry,
Because you are not sad. Now, by two-headed Janus,
Nature hath fram'd strange fellows in her time:
Some that will evermore peep through their eyes
And laugh like parrots at a bag-piper;
And other of such vinegar aspect

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